ESSAY

ON THE

ORIGIN, PROGRESS, AND PRESENT STATE

OF

GALVANISM:

CONTAINING

INVESTIGATIONS, EXPERIMENTAL AND SPECULATIVE, OF THE PRINCIPAL DOCTRINES OFFERED FOR THE EXPLANATION OF ITS PHENOMENA; AND A STATEMENT OF

A NEW HYPOTHESIS.

HONOURED BY THE ROYAL IRISH ACADEMY WITH THE PRIZE.

Desine quapropter novitate exterritus ipsa
Exspuere ex animo rationem : sed magis acri
Judicio perpende : et si tibi vera videtur,
Dede manus : aut si falsa est, accingere contra.

LUCRET. Lib. II.

Verba rebus proba. SENEC. Epist. 20.

BY M. DONOVAN.

DUBLIN:

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1816.

TO THE RIGHT HONOURABLE

THE EARL OF CHARLEVILLE,

PRESIDENT

OF THE

ROYAL IRISH ACADEMY,

FELLOW OF THE ROYAL SOCIETY, AND F. A. S. *&c.*

My Lord,

Sentiments of respect long entertained in private, and an acquaintance with your Lordship's attainments in the branches of knowledge comprised in this Essay, have been my inducements in offering to your Lordship this public although trifling tribute.

I have the honour to be,

My Lord,

Your Lordship's faithful Servant,

M. DONOVAN.

DUBLIN, 1816.

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PREFACE.

DURING the few years that I have turned my occasional attention to the consideration of Chemistry, and of those subjects connected with it, I was impressed with the unsettled and unsatisfactory state of that branch called GALVANISM. Unfortunately for the sciences, the impatience of human nature is unfriendly to the slow but certain process of collecting general principles, by abstraction from a numerous catalogue of well authenticated facts: and of this, the history of Galvanism presents an egregious instance. Scarcely was the first fact discovered, when an hypothesis was invented, which being applicable only to what was known, possessed but a short sway over opinion. The consequence, as might be ex-

pected, was that a few succeeding observations subverted the baseless system, and proved how extravagant are the wanderings of Imagination, when uncontrolled by the dominion of Reason. Numerous hypotheses have been since devised, but from the continual accession of new facts, none have continued applicable beyond the period of their discovery. The general proposition, that galvanic phenomena are occasioned solely by the agency of electricity, has been however, from the commencement, received amongst philosophers: they have supported their hypotheses by analogies and facts which, although ingeniously conceived and applied, to me never appeared conclusive. In this state of my opinions, the question proposed by the Royal Irish Academy, in 1813, brought my attention more particularly to the subject; I presented them with the result of my reflections in an essay, upon which they were pleased to confer the prize. In this essay, many alterations are since made; I have both added and expunged: and as I (perhaps vainly) supposed that some of my reasonings and experiments might contribute

to elucidate certain obscure points in Galvanism, I chose rather to publish the whole in one connected series, than to destroy its interest by the frequent interruptions unavoidable in a periodical journal. To those acquainted with Academic usage, I need scarcely observe that had I published this essay even in the form in which it was originally presented to the Academy, that learned body could not be considered accountable for the reasonings made use of. Mush less then are they accountable for many peculiar opinions at present inserted, which as yet they have never seen.

In publishing this essay, I hope it will appear that my design was not to make "a book," as it is commonly expressed. I have been careful to omit every thing that did not contribute to the proposed end; and every thing admitted is treated of in as summary a manner as was consistent with perspicuity. I have passed rapidly over the endless, and now no longer interesting, details that have been made with regard to the minute circumstances attendant on muscular contractions. I have

entirely omitted reiterated statements, unimportant controversies, the matter of histories and reviews, investigations unsuccessful in their object, insignificant theories, and vague conjectures. Nothwithstanding my desire to render every one his merits, should any omissions occur which cannot be ranked under any of the foregoing classes, I hope the failure will be attributed either to oversight, or to limitation of knowledge resulting from a want of sufficient works of reference.

Being prevented by my limits from giving detailed accounts of the labours of the different philosophers, I have endeavoured to remedy the defect by giving, what I consider indispensable, copious references to where the original papers may be found.

As to the arrangement of the historical sketch, I have given every thing in the order of its discovery, unless where the difference of time was so inconsiderable as to admit of being sacrificed to convenience. But when the right of priority appeared an object, I have been critical.

The arrangement, in the order of their discovery, of so great a number of dissimilar facts, condensed into so small a compass, and passing in such rapid succession, may have introduced an appearance of hurry and indistinctness. Nevertheless I have been at some pains to connect them in such a manner as to make them flow in a somewhat easy and natural order, to give the whole an air of unity, and to bring it into the form of a history rather than into that of annals.

In discussing the various hypotheses, I have not been deterred from calling in question, although I hope with becoming deference, the opinions of some of the most distinguished philosophers of the day. In experimental branches of knowledge, authority does not afford conviction, it is destructive if allowed to operate, and in all cases requires scrutiny careful in proportion as its origin is great.

The publication of this essay has been protracted much longer than I expected. It was received by the Academy in the beginning of January 1814: I did not again

get possession of it until February 1815; and since that period I have been occasionally occupied in going over the experiments, and making various alterations and additions.

DUBLIN, MARCH 1816.

INTRODUCTION.

THE close of the eighteenth century seems to have constituted one of the most brilliant eras in the history of physics. Philosophers had long before completed a system of knowledge, relating to aggregate masses, stupendous in its fabric, beautiful in its parts, but as yet imperfect in not involving an acquaintance with the constitution of matter. They cultivated nature in motion, but neglected nature at rest; and while moving masses engrossed undivided attention, the quiescent properties of their atoms remained unnoticed or unknown. The eagerness of research seemed at length to consume in its own ardor; the inquirers were exhausted, although not the subjects of inquiry; and a love of novelty, one of the first springs of human action, allured men to enter into a new region of research, where every object was unknown, and every step encumbered with difficulty and uncertainty. Philosophers began now to feel that, however worthy of contemplation might be the immense planet surrounded by its sphere of attraction, the minutest atom was an object

not less interesting, and not less possessed of that wonderous influence. The attractions and affections of the atoms of matter became therefore a subject of research; a new power was acquired over nature; bodies were formed by art, to the existence of which nature even seemed averse; and man was raised one degree in the scale of creation. Chemistry now flourished, yet experienced the vicissitudes of a science in its nature not demonstrative, but subject to opinion; until at length a constellation of philosophic genius appeared, which diffused on it a splendor that it never before possessed. While extensive influence was thus acquiring over inanimate matter, some facts of a singular and unexpected kind were observed, which led men to suppose that equal power might be gained over beings of organic structure. It was found that an appearance of life might even be excited in animals after their natural life had been extinguished. These extraordinary facts, originating in accident, were prosecuted with the energy which their surprising nature demanded; they led philosophers to the knowledge of a power which promised the most important discoveries, and afterwards fulfilled them even beyond expectation. The fortunate individual who commenced this interesting career, has his name perpetuated by being identified with the branch of knowledge which resulted: and while ever Galvanism is considered deserving of the attention of mankind, the name

of Galvani will be remembered with gratitude and respect.

To give a summary yet distinct survey of the present state of this new branch of knowledge, and to connect it with that science, of which I consider it a part, by shewing that in both, the agent is the same, although differently exerted, is the proposed end of this essay. Its object will therefore be to trace the progressive developement of Galvanism, from its commencement to the present day; to investigate, by reasoning and experiment, the principal hypotheses which have been constructed to explain its phenomena; and to present the outline of a new doctrine, founded on some laws of chemical attraction not hitherto developed. These three objects constitute also the basis of division; the whole essay being divided into three parts. The first contains a sketch of the history of Galvanism, subdivided into four periods. The second contains examinations speculative and experimental of galvanic hypotheses. The third gives a statement of the new hypothesis, with its application to the principal phenomena. As to the arrangement, I conceived it the best method to present, in the first part, a survey of the early opinions, and to point out the various modifications which they underwent, until they assumed the form in which we, at present, find them. By this method, the subjects of the second part become more clear; we obtain a better acquaintance

with the grounds upon which the different hypotheses have been founded; and can therefore more fairly appreciate the arguments adduced against them. After detailing the considerations that induced me to call in question the adequacy of these hypotheses, it remained, in the last part, to present a statement of the new views by which I hoped to explain the different phenomena in a manner less encumbered with difficulty.

Concerning these views I shall make little comment. I have only to hope, that whoever attends to the suggestions which I have brought forward, may be uninfluenced by his present opinions, in order that he may the more fairly judge whether there be any probability in mine.

ESSAY

ON

GALVANISM.

PART I.

Sketch of the History of GALVANISM.

THE phenomena of Galvanism naturally divide themselves into three classes. First, the phenomena of its physiological effects upon animals: Second, of its physical and chemical effects upon inorganic matter: Third, the phenomena discovered by the application of a general principle, derived from a knowledge of its chemical influence. These classes of facts having been discovered, with regard to time, in the order above specified, afford three epochs in regular succession. But previously to the true era of Galvanism, observations were made at different times, which, although they contributed nothing towards the grand discovery, must nevertheless be considered as facts of the same kind: they were such as would have perpetuated the name of any one possessing sufficient acuteness and perseverance

to scrutinize and extend them. This therefore constitutes an additional period; and, with the other three, completes the history of Galvanism; and from this basis we obtain the four following subdivisions. The first period contains those unconnected phenomena which must be considered as only precursory to the true era of Galvanism. The second includes the time between the discovery of muscular contractions, effected by the contact of simple metallic associations, and the discovery of combined galvanic arrangements. The third contains the gradual developement of the physical and chemical powers of combined galvanic arrangements. The fourth, and most important, comprises generalizations of the chemical effects of Galvanism, and the discoveries that have resulted from the application of a general principle. Such are the objects of the first part of this essay.

PERIOD I.

Containing Phenomena precursory to the era of Galvanism.

The facts of this period are of remote date, numerous, and so striking that it is surprising how they remained unconnected for such a length of time.

As far back as the records of observation extend, it has been known that the fish, afterwards

called the Torpedo, possessed the extraordinary property of benumbing the hand applied to it, by a sudden shock, which was supposed by the ancient philosophers to be a mode of defence bestowed on it by nature. Fishermen were said to have received the shock even through the line and rod. This singular animal occupied the attention of ingenious persons in all ages; but until the modern investigations of Walsh and Hunter,* little light was diffused on the subject. Several other kinds of fish have been also found to possess the same property; and it has been of late generally admitted that the agent in such animals is the same with that exerted in galvanic phenomena. This is therefore an animal galvanic battery known more than two thousand years.†

A series of phenomena of more modern, yet still remote date, is the conversion of metals into each other, long pretended by the Alchemists. This in many cases was no more than the precipitation of a dissolved metal by another, which therefore became coated, and apparently converted. It is now supposed that this effect is produced by a galvanic cause; and it will appear in the sequel that the supposition is more probable than even is yet known.

Anno 1700. The next phenomenon noticed was as obviously and decidedly galvanic as that which led Galvani to his grand discovery: yet

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* Philosoph. Trans. 1773.

† The Torpedo is first mentioned by Aristotle.

such was the unobservant habit of these days that it seems to have been forgotten almost as soon as known. M. du Verney observed that in irritating with a scalpel those nerves of a recently killed frog which supply the thighs and legs, the parts below suffered convulsions.* It is surprising that this fact did not place the era of Galvanism at a much earlier period.

1766. The organs of motion in animals were at this time found to be contractible by the action of common electricity; but the fact seems to have excited no attention. Dr. Priestley having passed a large electrical discharge through the body of a rat, the animal instantly died, after suffering a universal convulsion.†

1767. The organs of sensation were also found to be affected by artificial means; but a chimerical hypothesis baffled the prosecution of the discovery. Sultzer, a Prussian philosopher, observed, that if a piece of silver, in contact with a piece of lead, be applied to the tongue, a peculiar taste is perceptible, although neither metal separately can produce the effect. This was a distinct galvanic phenomenon which might have been the pole-star of discovery: but Sultzer explained it in the philosophical jargon of his day; and the world, as well as himself, were satisfied with an explanation not less obscure than the fact.‡

* Memoires de l'Academie Royale des Sciences. An. 1700. p. 52.

† History of Electricity, 4to edit.

‡ Theorie des Plaisirs, p. 155.

A fact of the same kind had been also noticed, and is still commonly believed, namely, that ale and similar liquors have a better flavour when drank out of a metallic than out of any other vessel.

1786. A circumstance is on record which would appear to shew that other animals beside fish possess an active electric organ. A medical student was occupied in dissecting a mouse; on applying his scalpel to one of the nerves, he received an electrical shock, which stunned him for a quarter of an hour. The shock extended as far as the neck, and was attended with a painful sensation in his arm, and a giddiness in the head which continued for some time. This fact gave origin to many conjectures concerning the nature of the power concerned; many supposed that the agent was the nervous fluid, which they identified with electricity. It is singular that this phenomenon has never occurred to any other person. It appears more probable that the sensation experienced by the medical student was the effect of some natural convulsion.

1787. A fact was now ascertained which, had it been connected with Sultzer's experiment, would at once have afforded the explanation, at present received, of the peculiar taste excited by the application of the tongue to dissimilar metals in contact. The Rev. A. Bennet found that certain metals, after contact with each other, became feebly but distinctly electrical.* The same thing

* Philosoph. Trans. 1787.

was afterwards shown more extensively by the experiments of Cavallo.* Yet these experiments seem for some years to have been forgotten, as they have never been adduced in defence of the hypothesis of Galvani, notwithstanding the great support they would have offered to that doctrine.

Thus it appears that, during this first period of the history of Galvanism, some of the principal facts had been witnessed without being connected or recognized. The contact of dissimilar metals was found to liberate electricity; the same cause appeared capable of affecting the organ of taste, in a peculiar manner: metallic irritation was also found capable of causing muscular contractions in animals; and common electricity was observed to produce the same effect. The chemical effects of Galvanism were also witnessed in a very striking degree. And finally, a very active galvanic battery was known, which was not only capable of giving a shock, but even a spark. Little else than these facts, in a more expanded form, is known in the present day; yet this period can be scarcely connected with the real origin of Galvanism. The foregoing phenomena, therefore, like those adscititious clouds which precede thunder, although light and unconnected amongst themselves, yet plainly foretold the sudden and splendid illumination that was to follow.

* Complete Treatise, vol. 3. p. 111.

PERIOD II.

Containing the time included between the discovery of muscular contractions effected by simple metallic associations, and the discovery of combined galvanic arrangements.

Since the observation of the first truly galvanic phenomenon, a century had now elapsed. The fortunate recurrence of the same fact, under nearly the same circumstances, formed the real nucleus round which all future discoveries collected, and the prosecution of this subject constitutes the real era of Galvanism.

Anno 1791. Galvani, an Italian philosopher, had been, for a length of time, engaged in making experiments to illustrate a favourite hypothesis. He conceived that the cause of muscular motion is electricity; an opinion to which certain hypotheses of that day led, such as the identity of the nervous and electric fluid: and a curious coincidence of circumstances brought a fact under his observation, which he considered almost tantamount to a demonstration. It happened that some dissected frogs lay on his table in a room where an electric machine was in action. Galvani's knife touching a certain nerve of the animal, while a person present drew a spark from the machine, contractions were immediately produced in its limbs. This effect he at first conceived to be owing to the accidental wounding of the nerve; but

other trials convinced him that the case was otherwise. Recollecting the spark, he desired it to be again drawn, while he touched the nerve with his knife; the contractions were again produced: and he could renew them as often as he pleased, provided that the spark and contact were simultaneous, and that he held the metal part of the knife. This experiment he varied many ways, and at length thought of trying atmospherical electricity. The nerves of frogs were connected with a conductor raised above his house, the muscles of their legs communicating with the ground. When an electrified cloud passed over the conductor, the frogs suffered convulsions. Even when the frogs were suspended from the iron palisades of his garden, convulsions were frequently produced. These he at first attributed to atmospherical electricity; but he soon found that when the animal lay on an iron plate in his room, the same contractions were produced if some other part were touched by a metal, in contact also with the iron plate; especially if that metal were silver. He also produced contractions in other animals by coating some principal nerve with metallic foil, and forming the communication between this armature and the dependant muscle by any conducting arc.

The hypothesis invented by Galvani to explain these phenomena was, that there is a quantity of electricity secreted by the brain, and inherent in the muscles of animals; that this

has a constant tendency to pass from one part of the animal to another, or from one part of a detached limb to another, as in the case of the Leyden phial, which he considered a direct analogy. The inner parts of a muscle are positive, the outside negative, or vice versa; and the nerve acts no more than the part of the wire connected with the inside of the Leyden phial, serving to discharge the internal electricity of the muscle. The electricity thus passing, acts as a stimulus to the muscle, and therefore causes it to contract. In corroboration of this supposition, he found that the metallic arc was not indispensable; for by merely applying the lumbar nerves to the crural muscles, perceptible contractions were produced. The effect was more powerful when a metallic arc was employed; but considerably more so when the arc consisted of two metals.*

Facts and opinions so extraordinary could not fail to excite general attention: and a numberless variety of experiments relating to this novel subject were undertaken, and various opinions formed, by almost all the philosophers of Europe. In this, as well as in all other new inquiries, it happened that many of the facts accumulated were mistaken, many contradictory, and many calculated only to support the peculiar views of the inquirers. Hypotheses were founded upon experiments of so uncertain a

* Aloysii Galvani de viribus electricitatis in motu musculari commentarius. Bononiæ 1791. This is the title of Galvani's work.

nature, as never to succeed in the hands of others, and seldom in those of the same person. Some concluded boldly from facts that were either equivocal or of no force; and others embraced opinions which their own experiments should have taught them to reject. Amongst the numerous inquiries of this kind, I shall select those alone which eventually increased the knowledge of the new subject of investigation.

1792. The extraordinary opinions of Galvani were embraced by many: others rejected a part, or modified the whole, each according to his own genius. The great Spallanzani admitted that the cause of the contractions might be electricity, but he reasonably enough chose to derive it from external bodies, rather than to suppose it inherent in the animal. To remove this objection, Galvani repeated the experiment so as to avoid every possible source of external electricity: he dissected the animal without even touching it with a conductor; he applied the exciting arc out of the influence of the atmosphere, but with the same results. Carradori, who conceived the electricity to flow from the metalic armature, became a convert on being shewn that the armatures might be omitted. Humboldt and others objected to Galvani's doctrine, on the grounds that electricity and the cause of contractions are not transmitted by the same bodies. Professor Pfaff considered that the agent in Galvanism is not electricity, but one *sui generis*; that the metals are the conductors: and that this agent is either associated or identical with the

principle of life. He showed that Galvani's fundamental principle, relating to the difference of electrical state between the inside and outside of the muscle, is gratuitous. Fontana even denied that the principle of muscular motion is in any degree connected with electricity.

1792—3. Dr. Valli adopted the electrical hypothesis of Galvani, except with regard to the surfaces of the muscle supposed to be occupied by the two states. He considered electricity as the real cause of the contractions, and as identical with what, in the physiology of that day, was called the nervous fluid: and of this he adduced the following evidences. Both fluids are conducted by the same bodies, and non-conductors obstruct both. Non-conductors when strongly heated obstruct neither; and water by the operation of cold, becomes a non-conductor to both. Attraction is a property of the nervous fluid, as well as of electricity. At the instance of Dr. Valli, the commissioners of the French Academy of Sciences "placed a prepared frog in a vessel which contained the electrometer of M. Coulomb, charged negatively and positively by turns. In both cases, in exciting the animal in the common way, the ball of the electrometer was attracted." The permanent disturbance of the electric equilibrium he compared to the difference of temperature maintained by an animal when immersed in a temperature higher or lower than its own. Notwithstanding the great sensibility of the muscle to its natural electricity, he found it much

less so to that which was artificially excited: frogs with coated nerves were not affected by strongly excited wax, nor by glass. He considered two metals necessary for producing contractions, and when one acted, he thinks that there must have been a difference of quality in some part: for two qualities of lead produced the effect, although not if they were the same. He found that opium applied to a nerve diminished its excitability, yet not permanently: its effect extends upwards towards its source, but not downwards towards its termination. Animals killed by mineral poisons, by starvation, or by internal gangrene, afforded no contractions; and those destroyed by mephitic air, scarcely any. In some instances drowned animals were resuscitated, other trials failed. He made experiments on frogs, lizards, eels, larks, kittens, dogs, horses, tortoises, fowl, and rabbits, and found that the metals produced contractions in all. He maintained that the muscles of involuntary motion are not excitable either by the metals or by common electricity.*

That the nervous and electric fluids are identical was not an opinion originating with Valli; it had been maintained by Bridon, but never gained much popularity. Both this view and that given by Galvani were immediately opposed by Dr. Monro of Edinburgh. He could

* Experiments on Animal Electricity, with their application to Physiology. London 1793.

neither suppose that the agent in contractions is electricity, nor that it is identical with the nervous fluid. He conceived that the contact of the metals with the nerves evolves some fluid, which is no more than a powerful stimulus to the nervous fluid, and which thus produces contractions.*

1793. These and numberless other facts, most of which were crude and uninstructive, were accumulated in the short period of two years: and from the limited object of the new branch of knowledge, it was termed animal electricity. In the same short period, no less than three hypotheses had arisen. First, that which denied the agency of electricity in muscular contractions; second, that which asserted the agent to be intrinsic electricity; and third, that which maintained it to be extrinsic electricity. Volta, a philosopher of Pavia, who had already distinguished himself by his labours, had been for some time engaged in this investigation. In none of the foregoing opinions could he coincide; for although he agreed, with Galvini, that the agent is electricity, yet he could not allow its production to be owing to the animal, nor its unequal distribution on the surfaces of the muscle. He found that the supposed restoration of the equilibrium between the surfaces is not always necessary; for contractions could be equally produced by making the cir-

* Edinburgh Transacdons, vol. 3. p. 251.

cuit between two parts of a nerve only, or between two muscles, or between different parts of the same muscle: but in these cases, the application of two metals was always necessary. Volta therefore argued that, if ever Galvani's supposed discharge take place, there are cases in which the contractions are owing to a quite different circulation of the fluid, since with different armings on the muscles or nerves alone we can produce them. Indeed, he observed, we should say, that we disturb rather than restore the equilibrium; and this by an action proper to the different metals. Thus he affirmed that he had discovered a new law, not of animal, but of common electricity. For the production of the contractions, he observed, that quantities which did not affect Bennet's electrometer, and barely influenced his condenser, were sufficient: hence he was not surprised that the distant sparks from the electric machine affected the frogs in Galvani's experiment: and hence, from the great sensibility of the frog, he denominated it the animal electrometer. Finding contractions so readily produced in dead animals, he wished to try its effects on the living. For this purpose he made an experiment on himself; and as the tongue is the part least covered with integuments, he subjected it to the action of two different metals, viz. zinc and silver. Instead of motion, a peculiar taste was excited, and this surprised him until he reflected that the nerves in that part of the

tongue are calculated for sensation, and not for motion: the latter object was however accomplished by acting on the nerves of motion in the root of a tongue recently extracted from a sheep.*

It appears that in this paper, Volta invalidated the hypothesis of Galvani concerning the analogy of the Leyden phial, by shewing that when two parts of the same nerve are acted on by two different metals, in contact, contractions follow. But if the contractions in this case result from the electricity evolved by contact of two metals, it is reasonable to suppose that all other cases of contraction are owing to the same cause. Why then are contractions produced by one metal? At this time Volta thought of no other kind of combination than that of different metals. Here therefore is the defect of his opinions at that period. They were considerations of this kind that very shortly after induced him to extend all the preceding views.

The hypothesis of Volta, now so universally admitted, was at that time far from being generally received. Amongst others, Vassali-Eandi, a philosopher of reputation, preferred the hypothesis of Galvani; but modified it according to his own conception of the phenomena. With Volta he denied the analogy of the Leyden phial, but admitted that the contractions are

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* Volta's First and Second Letters to the Royal Society. Phil. Trans. 1793.

produced by electricity inherent in animals, conducted but not generated by the metals applied. As to the origin of this electricity, he observed that chemical changes, as of air in respiration, food in digestion, &c. are continually going on in the system; and that in all chemical changes, there is a concomitant change of capacity for electricity. This is proved to have happened by respired air, and by blood just drawn, the former manifesting indications of negative electricity, the latter of positive. Thus the source of electricity is traced; the two states are shown to exist in different parts; and the metallic arc, by occasioning pressure on the nerves, facilitates the passage of the fluid; the case being analogous to the pressure observable in some muscles of the Torpedo when the animal is about to transmit the shock.

Even the general proposition that electricity is the cause of contractions did not yet meet general approbation. From the above modification, as well as from those of Volta, Valli, and Galvani, Dr. Fowler still dissented. He could not consider the phenomena of Galvanism as in any manner reconcileable to the established laws of electricity: nor did he even allow the circumstances necessary to the production of electricity to be present, as, to this effect, an excitation of an electric by a conductor is required. But in galvanic experiments, all the bodies concerned are conductors, and therefore there could be no disturbance of the equilibrium. As to the influ-

ence of the exciting arc, he affirmed that charcoal, which conducts electricity perfectly, is incapable of producing contractions. He did not allow that the *galvanic influence*, whatever it may be, is derived from the metals alone, but that the animal contributes, in part, to its production.

With regard to the muscles acted on, Valli, Volta, and others, asserted that in those alone which are subject to the will, contractions could be produced. Fowler with difficulty caused the heart to contract, but could not affect the stomach or intestines. The experiments of Marsigli, Humboldt, Grapengiesser, Smuch, Fontana, Giulio, Vassali-Eandi, and Rossi, however, proved that contractions could also be produced in the stomach, intestines, bladder, and vessels. Aldini showed that they could be produced as well in vacuo as in air: he also transmitted the galvanic agency through 200 feet of water, and found that it produced the effects on animals, although in a somewhat less degree.*

Fowler also found that earth-worms and leeches are very susceptible to galvanic stimulus; a fact which he considered as proving that these animals are supplied with nerves, although this was contrary to the received opinion. He observed also that muscular contractions are more easily produced if inflammation be going on. He and Robison observed the flash produced by the contact of different metals in the mouth.†

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* Aldini on Galvanism, p. 217.

† Fowler on Animal Electricity, 1793.

1794. Volta had already declared his opinion that the electricity, which in Galvani's experiment produced contractions, was evolved by the contact of the metals. This opinion he continued to illustrate and extend. He found that if a tin basin, filled with alkaline liquor, be grasped with wet hands, and the tongue applied to the fluid, an *acid* taste is perceived. He supposed electricity in this case to pass from the tin to the liquor, from thence, through the tongue and body, to the outside of the vessel. He divided conductors into dry and moist, the first comprising metals, certain minerals and charcoal: the second containing certain fluids, and bodies moistened with them. He now remedied the defect of his former views by stating, as a general principle, that the contact of a conductor of the first class with one of the second, disturbs the electric equilibrium: and that this power is different in different conductors. In a perfect circle of conductors, where either one of the second class is placed between two different ones of the first, or one of the first between two different ones of the second, an electric stream is occasioned by the predominating force either to the right or to the left; this circulation ceasing only when the circle is broken. When the circle consists of two kinds of conductors, however different or numerous, two equal powers are opposed to each other, and no electric stream is produced. There are other combinations where the powers are in equilibrio, and no current takes place, notwithstanding the inter-

vention of several different metals. Such is the case when each of these metals is placed between two moist conductors of very nearly the same nature: or when, in a circle of three pieces, two of the same metal are separated by one of a different kind. For a circulation of electricity it is absolutely necessary that two different conductors of the first class should be, on one side, in contact with each other, and on the other, with conductors of the second class. With regard to the experiment of the tin basin, Volta afterwards found that a strong galvanic arrangement is formed when the basin is silver and when the liquid is solution of sulphuret of potash.*

1795. The hypothesis of this philosopher, which supposes that the cause of the animal contractions is the restoration of the equilibrium between two metals, in which it had been disturbed by contact, was called in question by Dr. Wells, on the grounds that the animal fluids should have established a sufficient connection between the metals without any other, and that therefore the disturbance of the equilibrium might have taken place without direct contact of the metals. He also opposed the explanation given of contractions by Galvani; for if they be owing to the restoration of the equilibrium between the internal and external parts of the muscle, any conductor applied to the nerve and muscle should produce

* Volta's Letter to Gren. Phil. Mag. 4. 59.

contractions: and Volta affirms that this is really so. But Doctor Wells held the nerve of a newly killed rabbit in one hand, and with the other touched the muscle, yet no contractions ensued; nor did any happen when he caused the nerve, held by a non-conductor, to touch the muscle directly. He denied that the nerves and muscles have any share in the excitation of the galvanic influence, and shewed by experiment that they merely act by their moisture. He brought forward some experiments tending to prove that single metals, by being rubbed against others, acquire the property of producing contractions, and retain it for a long while. As to whether this influence be or be not electricity, he observed, we find them resemble each other so generally, that we infer their identity. Both are conveyed by the same conductors. Fowler objected that charcoal did not conduct the galvanic influence, but Dr. Wells found that this was a mistake, for with zinc it excited even as strongly as gold; but all specimens did not act equally, and long keeping diminished its power. It is however to be observed, that two years before, the conducting power of charcoal appears to have been known to Marsigli, Aldini, and others.* Dr. Wells found that the fluids capable, with metals, of exciting the influence, are water, vinegar, and the mineral acids; alcohol acted feebly; and those

* Aldini, 153.

which are non-conductors of electricity, as fat and essential oils, and ether, did not produce any effect. Nevertheless he did not conceive that the electricity is in both cases subject to the same laws.*

Indeed the difference of habitude between the galvanic agent and electricity was too obvious not to be generally observed; many intelligent persons found it impossible to give assent to their identity; and hypotheses of a very opposite nature were still devised, and found followers. A little before this period arose the chemical hypothesis of Galvanism; it originated with Creve, and in it he seems really to have anticipated subsequent discoveries. Creve's hypothesis was, that galvanic irritation depends on chemical action: that by means of two metals, or one with charcoal, the water of the nerve and muscle are decomposed: that the oxygen is attracted either to the metal or to the charcoal, and therefore forsakes the hydrogen. He mentioned that if a metallic apparatus be immersed in a glass of water, the water is decomposed, and that if the tongue be applied, it will be sensible of it. These singular assertions seem to have been little regarded: and from the vague manner in which they were promulgated and investigated, they diminish nothing from the merit of the discoveries afterwards made by Nicholson and Carlisle.

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* Wells Phil. Trans. 1795. 246.

1795—6. The chemical nature of the galvanic stimulus was maintained, in a manner somewhat different, by the celebrated Humboldt. After a number of experimental inquiries, during which he ascertained many new facts, he declared, with a precaution and hesitation becoming such a philosopher, and well worthy of imitation, that he could not pretend to determine what might be the nature of the fluid which produces these astonishing results. Its effects, he observed, are always painful; the pain encreasing until the nerve become insensible from continued stimulus: it does not resemble electricity. He related an experiment in which he blistered the parts over the deltoid muscle of each arm, and having applied a piece of silver over one sore, zinc over the other, and having connected the metals by means of silver, a serous fluid immediately poured out, which in some seconds became dark coloured, and left traces of inflammation wherever it passed. He found that if the thigh of a frog be thrown into oxymuriatic or nitric acid, it remains motionless, but if then transferred to solution of potash, it suffers contractions as strong as metals could produce. He also steeped the legs of frogs in alcoholic solution of opium; when taken out, the application of metals produced no effect: but one leg being thrown into water, and the other into oxymuriatic acid, the former remained motionless, but the other was thrown into strong convulsions. A frog's thigh moistened with solution of potash, and laid on glass, fell into

convulsions without the application of metals. And the heart of a fish, which had entirely ceased to palpitate, again palpitated when thrown into oxymuriatic acid. By the application of the galvanic stimulus, he roused the vital energy of a dying bird, so that it continued to live for many minutes longer.

Humboldt considered galvanic phenomena as not owing to electricity, but to some fluid inherent in living animals, analogous to magnetism and electricity. He instanced the great dissimilarity of the two sensations. The irritability of fibre he attributed to the equilibrium of its elements, azote, hydrogen, carbon, oxygen, sulphur, phosphorus, &c. He considered that chemical substances act by increasing irritability; and he supposed that there are other substances which diminish it.* In many of his experiments, however, in which acids and alkalies seemed to increase irritability, metals were employed; and therefore the effects might be owing rather to the action of the chemical substances on the metals than on the nerves.

This was probably one of the most important investigations that had been undertaken since the discovery of galvanic contractions. It was known from the researches of Haller and others, that acids, alkalies, and other substances, when applied to muscle, cause it to contract. The experiments of Humboldt formed the link of connec-

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* Annales de Chimie, tome 22. 51. and Nicholson's Journal, 4to. vol. 1. 256 and 359.

tion between the old and new physiology; they ought to have shown that electricity is not the sole agent in galvanic phenomena. From this consideration, as well as from examining the productions of that day, it appears that the labours of Humboldt were not estimated according to their value. However it cannot be denied that, as Fourcroy observed, the composition which he attributed to certain chemical compounds would have required more experimental evidence, before assent could be given to some of his inferences.

1796—7. The electrical hypothesis, nevertheless, still continued to gain adherents. We have seen that, according to Galvani, the electricity which causes contractions resides in the animal; and according to Volta, it is excited by the disturbance of equilibrium which happens when conductors of a different kind are brought into contact. Volta now showed, by an experiment of great delicacy, that during such contact, electricity is really and sensibly produced. He found that by bringing together two insulated plates of different metals, electricity could be rendered manifest by means of Nicholson's doubler; or better, by his own condenser. After applying one of the insulated plates 20 or 30 times to a small Leyden phial, the latter became charged, and gave indications of its charge to the electrometer.* This experiment, at the same time that it established one of the chief principles of Volta,

* Annales de Chimie, tome 29. 91. and tome 40. 225. The former decides the date.

entirely subverted the hypothesis of Galvani: it was the fortunate fact that afterwards led to a discovery which has perpetuated the name of Volta in the annals of physics.

Notwithstanding the great accession of strength which accrued to the opinions of this philosopher, the chemical hypothesis began to obtain a higher degree of popularity than ever. As early as 1792 the efficiency of electricity in producing contractions was called in question by Fabroni; he sent an essay on the subject to the Academy of Florence, which by some means was not then given to the world; but about this period (1797) he gave a new account of his inquiries.* The chemical hypothesis seems to be the prior and exclusive right of Fabroni; he gave the first account of it; he applied it to phenomena with a known ingenuity: and it may be remarked that the views of Creve are of so different a nature as to entitle him also, as far as he went, to the full merit of originality.

The power which produced the taste in Sultzer's experiment, Fabroni considered as the same with that which occasions contractions; it not being in either case dependent on electricity, but on chemical action. The nature of this chemical action he explained in the following manner.

He observed that mercury, tin, lead, and copper, when pure, remain a length of time un-

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* Journal de Physique, tome 6.

changed; but when alloyed, they are oxidated in a comparatively short period. On this account, certain Etruscan inscriptions, of the most remote antiquity, are in high preservation, because engraved on pure lead; while the pontifical medals, although of a much later date, had almost crumbled into white powder. On the same account, the copper sheathing of ships is found to be completely corroded where iron nails pass through. Hence he concluded that metals exert a mutual action; and to their contact, these effects are to be attributed. The agency of contact he explained as follows:

Metals are known to combine by mutual solution. The tendency begins when the particles come into contact; cohesion is the obstacle; and fire acts only by overcoming this obstacle. Hence metals, by exercising their mutual affinity, must have their respective powers of aggregation diminished: and although, while separate, their cohesion would prevent their attracting oxygen from air or water, yet by contact they may acquire that power, as they pass to new combinations. Probably therefore, he observed, some of the effects on animals may be attributed to the chemical operation of the armature, to the transition of oxygen into a combination, to the formation of a new compound, or to the developement of a soluble or sapid taste.

That metals having a mutual affinity will, by contact, weaken each others cohesion, thus causing them to yield to the action of the weakest sol-

vents, he showed by the following experiments. A number of glasses were filled with water; into each was put a metal of a different kind, as gold, silver, copper, tin, lead. In a second set of glasses, two metals, separated by a slip of glass, were immersed; one being mere oxidable than the other. In neither of these arrangements did any change take place: but when the experiment was repeated as in the last case, except that the slips were omitted, the plates being therefore in contact, the more oxidable metal was soon loaded with oxide, and even crystals were formed. Thus there was an evident chemical action,—an oxidation or slow combustion of the metal, which must have been attended with a developement of light and caloric. That caloric is evolved by the action of metals on each other, is shown by amalgamating gold with mercury; and that light results, may be ascertained by placing a bit of tin over the ball of the eye, and a piece of silver in the mouth: when the metals are made to communicate, a flash is perceived. The case is a slow combustion of the metal, and on this depend the sensations. The stimulus may therefore be caloric, oxygen, or the metallic salt: but which, he did not pretend to determine.

It had been first observed by Professor Robison,* then without a knowledge of Robison's experiment, by Dr. Fowler, and afterwards by

* Supplement to Encyclop. Brit. vol. 1. 682.

various persons, that when the mouth forms a part of the circuit between two metals, a flash is perceived. Fabroni considered this a deception, for he could see the flash only when the eye was made a part of the circuit. But here he was evidently mistaken ; and I have known many to whom the flash was not perceptible. Fabroni maintained that the light is not electrical ; he allowed that electricity is evinced in the separation of metals that had been in contact : he admitted it as an effect, but not as a cause : and he remarked that many chemical operations, as volcanic eruptions, melting of sulphur, and evaporation of water, are attended by electrical appearances. With regard to the opinion of Galvani, Aldini, and Volta, who supposed the agency in galvanic phenomena, on account of the quickness of its transmission, attributable to electricity alone, he observed that chemical action operates with the rapidity of lightning. During the mutual action of the metals on water, he found that air is absorbed, that a film of oil on the water obstructs the process, and proved that this acts by excluding air, and not by excluding electricity. He found also that the necessary oxidation of the metal takes place without air, if oxygen can be supplied from any other source; thus when a plate of iron is laid on red lead in water, the iron is oxidized without decomposing the water.*

* Nicholson's Journal, 4to. vol. 4. 120. Although Fabroni's first paper

Such are the principal facts and conclusions contained in the Florentine philosopher's paper; a paper eminently distinguished by ingenuity, and by the beautiful simplicity of its inferences. The diminution of cohesion by the power of affinity; the consequent susceptibility of one of the metals to the action of the weakest solvents; the formation of a metallic salt, producing the taste; the production of the flash in consequence of the mutual action of the metals; all seem to be happy conceptions, and to afford explanations more consonant than any other to the state of knowledge at that period. It is a doubt that even the refined speculations of our own day have afforded us any opinions so reconcileable to reason. And in the sequel, we shall see additional reasons for supposing Fabroni to be right with regard to the dependence of galvanic phenomena on chemical action; although there are no grounds for admitting oxygen or caloric to be the immediate agent.

Fabroni seems to have derived his supposition, concerning the mutual action of metals, from observation of facts, and to have been unacquainted with an experiment made by Dr. Ash. This gentleman found that zinc, if in contact with silver, is more speedily oxidated than

was read to the Academy of Florence, 1792, in his second, from which I have collected the above, he alludes to the experiments of Volta, on which account I have placed his hypothesis at this period of the history.

if alone: from this, he, as well as Fabroni, inferred that galvanism is the result of chemical action. I know not which of these experiments had priority; but it appears that neither was acquainted with what the other had done; and therefore the merit of each remains undiminished.

Each of these different hypotheses had its followers. That of Volta, though as yet imperfect, was adopted by the generality of those who had maintained the opinions of Galvani. The general outline of Fabroni's doctrine was so strictly accordant with the facts then known, that it gained much celebrity, and even continues to intermix in the doctrines of the present day. Reinhold, an intelligent inquirer of that period, seems in part to have adopted the opinion of Humboldt. He considered galvanism as a fluid *sui generis*, having a close analogy to electricity and magnetism. He attributed its source and formation to the animal, on the grounds that when a nerve is brought in contact with the muscle to which it belonged, contractions are produced. This fluid he conceived to be secreted in animals alone, and by no other parts than the nerves.

1798. Reinhold was also the author of a masterly history of galvanism, which appeared during the year now under consideration. In this, he not only collected and arranged the labours of all the inquirers into the new branch of knowledge, but also added to it many inves-

tigations of his own. The work displays an accurate and extensive acquaintance with every thing then known, and may be considered as a secure *depôt* of every fact and opinion, from the first researches to the time of its publication. In the present day it might be looked upon as a book of little interest, perhaps of wearisome minuteness: its importance has decreased as the opinions which it contains became antiquated: and it is now to be valued rather as a work of reference than of use.*

The taste excited in the mouth by metals in contact, which Fabroni supposed to be produced in the same manner as contractions, had occupied the attention of Professor Robison, much about the same time with Fabroni (1792). Professor Robison went still further; he alternated a number of pieces of zinc and silver: he applied his tongue flat upon their edges, and felt an exceedingly unpleasant taste.† How much to be regretted it is, that the omission of a few slips of wet paper should have deprived so deserving a philosopher of a discovery which would have transmitted his name, with honour, to posterity. This discovery was reserved for one more fortunate, and not less deserving. Volta had for eight years continued his experiments, and had followed up his opinions concerning the electricity evolved by con-

* See a copious abstract of this work, Wilkinson's Elements of Galv. vol. 1.

† Encyclop. Brit. Sup. vol. i. 682.

tact. His exemplary perseverance was at length (1800) rewarded by the discovery of an extraordinary instrument, in which the electricity of metals is rendered manifest to every sense; which continually discharges that fluid, and is instantly recharged. This instrument he constructed by piling plates of zinc and silver, in contact, alternately with wet pasteboards of equal size, in the order silver, zinc, pasteboard, silver, zinc, pasteboard, and so on to any proposed extent. If the number of triads be twenty, the electricity is so strong, as not only to occasion a divergence in Cavallo's condensing electrometer equal to 10° or 15°, but also to afford sparks by the common condenser, and even, when a finger is applied at top and bottom of the pile, to give repeated shocks, resembling those of an electric battery weakly charged. These will be stronger if the fingers and hands be wet, and embrace a large surface of metal; and still stronger if, instead of water, the pasteboards be soaked in solution of salt, which acts better only by being a better conductor of electricity. Heat also seems to increase the power of the pile. It was justly a matter of surprise to Volta that this apparatus should be at one end in the negative state, and at the other in the positive, notwithstanding that it was entirely composed of conductors. On this account he considered that it has no analogy to the Leyden phial, but closely resembles the natural electric organ of

the Torpedo; the constituent discs of which he supposed sufficiently different in their nature to be good exciters of electricity, in the manner of the different metals of his pile. The latter he therefore called the artificial electric organ. He stated that as the metals of the pile are such perfect conductors, one point of contact will be sufficient: but it is otherwise with the fluids, which being imperfect conductors, require contact in a great number of points. He described another and more commodious form of the apparatus, consisting of a number of glasses filled with water or some saline solution, each being connected with the rest by a metallic arc, one end of which is zinc, the other silvered copper; the arcs being similarly arranged: so that zinc and silvered copper are in each glass, but not in contact. This produces effects precisely the same as those of the pile. The action of these instruments he supposed to depend on a simple principle discovered by him some years before, namely, that different metals, when brought into contact, become motors of electricity: and this electricity he found to have the power of irritating the organs of taste, of sight, of hearing, and of touch.*

He soon after found that if either metal in the pile have the same metal on each side of it, the effect is destroyed; for the influences of the ex-

* Volta's 3d Letter to the Royal Society, Phil. Trans. 1800. 403.

treme metals balance each other : but this balance is subverted if a wet cloth be substituted for one of the similar metals. By contact of a phial with a pile of 200 pairs during $\frac{1}{10}$th of a second, he obtained a charge which equalled that of the pile, and gave the same shock.*

If Professor Robison was unfortunate in missing the discovery of the pile, to which he made such near approaches, Volta was no less so in overlooking those properties of it which have since enlarged the boundaries of science. One possessed of more craft and circumspection might have concealed his invention, so as to secure to himself the results of its application. But Volta sought only the acquisition and diffusion of knowledge ; and superior to the vanity of wishing to be the sole organ of its communication, he gave to the world his discovery, left free the path to those who loved to search after truth, and threw open the gates of the temple of Science. Such was the discovery of the pile,— a column erected to the memory of its inventor, and inscribed with an epoch in the annals of knowledge.†

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* Pictet's Letter. Phil. Mag. vol. xi. 149.

† As a tribute of respect to the author of this great discovery, the National Institute voted to Volta a gold medal ; and the French government bestowed on him a present of 6000 francs. The intelligence was thus communicated to him :

" The French government, Citizen Professor, has granted to you a present of 6000 francs, (£250 sterling.) It has thought this mark of esteem due to the illustrious philosopher who, after enriching science with useful truths for 25 years, has come to deposit in the Institute the secret of nature, and of the

PERIOD III.

Containing the gradual developement of the physical and chemical powers of combined galvanic arrangements.

F Italy had to boast the discovery of metallic irritation, and of the electric pile, Britain has derived no less honour from the discovery of its control over inorganic matter. The names of Nicholson and Carlisle are made conspicuous by being the first observers of the chemical properties of the new agent; and by developing a power which has been the source of the brilliant discoveries that followed.

Anno 1800. Immediately when the accounts of Volta's pile were received in England, those gentlemen constructed a pile of 17 half crowns, as many pieces of zinc, and an equal number of cloths moistened with solution of salt. This pile gave a shock, and, if the skin were broken, a very acute sensation. It did not affect Bennet's electrometer, but the revolving doubler showed that the silver end was *minus*, and the zinc end *plus*.

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effects of Galvanism. I am happy in being the organ of government to a man whom I esteem, and for whom I have long entertained an affection. I beg you to accept the friendship of the Consuls. I salute you cordially.

(Signed) CHAPTAL,

Minister of the Interior."

When a drop of water was used to make one of the contacts certain, it was observed to effervesce, and to afford a gas which smelt like hydrogen. To collect the gas, they stopped a glass tube at each end with a cork, and through each cork passed a wire, the ends of which were kept at a certain distance from each other in the tube. The other ends of the wires being properly connected with a pile of 36 pairs, that proceeding from the silver soon began to give out bubbles in the tube, while the other was corroded and fell down the tube in the form of clouds. Hence they concluded that water was decomposed, that the gas evolved was hydrogen, and that the oxygen combined with the metal: but they were much surprised that the hydrogen should be evolved at so great a distance from the place where the oxygen appeared. When platina wires were used, both constituent gases of water were liberated; and by means of different vessels they were obtained separately, but proved not to be pure. These gentlemen ascertained several other facts: one of the principal was, that two piles of equal number, but of different surface, effected the decomposition of water equally; and on many accounts they concluded that number and not surface is required. When the interrupted circuit, above described, was made in muriatic acid, the negative wire gave out some hydrogen, the copper positive wire dissolved, but the metal was soon revived, and attached itself to the negative wire: the reduction was attributed to nascent hydrogen. When the interrupt-

ed circuit was made in tincture of litmus, the portion surrounding the positive wire became red. With regard to the decomposition of water, Mr. Nicholson observed that the same takes place between every member of the series; and that the salt is even decomposed, for an efflorescence of soda appeared round the edges of the pile.*

As soon as these remarkable results were made known, a number of inquiries were instituted by various ingenious persons. Mr. Cruickshank constructed a pile of an hundred pairs of zinc and silver plates, separated by papers soaked in solution of muriate of ammonia, which he found preferable to salt water. When he repeated the experiment of Nicholson, on the decomposition of water, using silver conducting wires, that proceeding from the zinc end dissolved and formed a cloud, which proved to be muriate of silver, the acid being derived from the muriatic salts present. When instead of common water, distilled water tinged with litmus or Brazil wood was employed, an acid and alkali were produced which he supposed to be nitrous acid and ammonia. He connected solutions of acetate of lead, sulphate of copper, and nitrate of silver with his pile, and in every case the metal was separated from its combination, and collected itself round the ne-

* Nicholson's Journal, 4to. vol. 4. 179.

gative wire. He repeated Nicholson's experiment with the copper wires and muriatic acid, only substituting silver wires and vinegar; but the result was the same. In decomposing water, Nicholson did not obtain the gases pure; Mr. Cruickshank also made trials, but for want of certain precautions, his resulting proportions were not strictly correct. He subjected many substances to the action of the pile, and effected their decomposition: and from all his experiments he drew up the following conclusions, which may be looked upon as the first step towards a generalization of the chemical effects of the pile.

1. That hydrogen with a little oxygen and ammonia are disengaged at the minus wire, whatever be the metal, provided the fluid acted on is pure water.
2. That when the fluid acted on is a metallic solution, the minus wire, instead of evolving hydrogen, reduces the metal.
3. That of earthy solutions, those of magnesia and alumina only are decomposed, a fact which favours the supposed production of ammonia.
4. That when the plus wire is of gold or platina, a quantity of gas is there liberated, equal to one third of the hydrogen evolved at the other wire. This gas is oxygen with a little azote. Some nitrous acid is also produced.
5. That when the plus wire is of an imper-

fect metal, a little oxygen is given out; but the wire is either oxydated or dissolved; sometimes much of it is oxydated, and little dissolved.

6. That when the gases, resulting from the decomposition of water, are exploded over mercury, nearly the whole volume disappears, water is formed, and probably a little nitrous acid.

Mr. Cruickshank also offered an explanation for the separate evolution of the gases from water, which was a phenomenon that justly excited general surprise. He supposed that galvanism may exist in two states, oxygenated and deoxygenated: that, in the decomposition of water, the influence passes from the silver end, seizes oxygen, and therefore liberates hydrogen; but on arriving at the zinc end, it liberates oxygen, which either escapes or combines with the wire.

One of Mr. Cruickshank's principal discoveries was a form of the galvanic apparatus, which combines neatness and convenience with a considerable increase of power. The trough is so well known that it needs no description.*

Mr. W. Henry of Manchester was also about this time occupied with galvanic experiments. He subjected sulphuric, nitric, and muriatic acids to the action of the pile; the two former were resolved into their elements; but the water alone of the latter appeared to be de-

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* Nicholson's Journal, 4to. vol. 4, 187. and 254.

composed. He made trial also of liquid ammonia, and of solution of potash, both of which he conceived to be decomposed; but in consequence of some subsequent suggestions, he, with a frankness which did him credit, allowed that he had been mistaken.*

Lieutenant Colonel Haldane constructed a series of forty pairs of zinc and silver, with discs of card moistened with water. This apparatus was placed horizontally on a table, and was so very feeble that it neither affected the most delicate kind of electrometer, nor gave a shock to wet fingers: yet it decomposed water. When immersed in water it entirely ceased to act; when taken out, and its external surface dried, it acted as before. It also ceased to act in a vacuum; but its original state was restored when air was admitted. He then erected a more numerous pile, in a vertical direction, and from this he obtained weak indications of electricity, but they diminished when an electric machine in action was connected with it. To account for the latter fact, Mr. Nicholson, commenting on these experiments, supposed that the electric stream from the machine might have been in a direction different from that of the pile; and he added, that galvanism resembles electricity in a state of low intensity. Haldane then endeavoured to ascer-

* Nicholson's Journal, 4to. vol. 4. 223.

tain the powers of different metals to produce galvanic effects. In the following summary, the experiments are arranged according to the degrees of influence which the combined metals seemed to possess.

Exp. 1. Zinc with gold, silver, copper, lead, tin, mercury.

2. Iron, with gold, silver, copper, lead, tin, mercury.

3. Lead with gold, silver, copper, tin, mercury.

4. Tin with gold, silver, copper, mercury.

5. Copper with gold, silver, mercury.

6. Silver with gold.

In a pile of two hundred pairs of zinc and silver, the power was much increased, when, instead of water, the card discs were soaked in solution of sal-ammoniac. Three different piles were placed under three jars; the first containing common air, the second oxygen, and the third azote. The action was strongest in the second, but in the third there was none; in the two former jars, the rise of the water indicated an absorption of oxygen. This latter fact, however, had been observed long before by Fabroni. On the whole, Haldane concluded, from the very minute and peculiar exhibition of the attractive and repellent powers of the pile, while the other effects are so powerful, that electricity is not the principal agent; and he conceived with Fabroni, that galva-

nic effects always depend on a chemical operation.*

Mr. Davy, who had already brought himself into high consideration by his chemical researches, now for the first time appears as an experimenter in this new field of inquiry. In this he also soon signalized himself; and by his characteristic ingenuity he extended not only the boundaries of galvanism, but of chemistry. He repeated the experiment on the decomposition of water, and by attention to various minute circumstances, he obtained the gases pure and in proportionate quantity. He varied the decomposition in several ways, sometimes causing oxygen to appear at the silver end, and the hydrogen at the zinc end; sometimes producing each gas respectively without the other; and at other times transmitting the influence without effecting any decomposition. These anomalous results will be more particularly stated hereafter, they therefore need not be here detailed. The conducting power of charcoal was discovered by Priestley; it was found by Aldini and Wells to convey the electricity supposed to produce muscular contractions; Mr. Davy found that it affords the shock and spark; and that it decomposes water; carbonic acid being, under proper circumstances, produced at the zinc end, and hydrogen, hold-

* Nicholson's Journal, 4to. vol. 4. 241 and 314.

ing a little carbon dissolved, at the other. He drew up some general propositions, which must be regarded as an extension of the opinion of Fabroni: they may be comprised in the following.

1. The pile is incapable of acting when the water between the pairs of plates is pure; for zinc is not oxydated at common temperatures by pure water.
2. The pile acts when the interposed water holds in solution common air, or oxygen, or nitrous gas, or nitrous or muriatic acid.
3. The power of the pile is proportional to the power of the interposed fluid to oxydate the zinc.

According to these principles, Mr. Davy constructed a pile, in which the fluid was dilute nitric or muriatic acid: the power was much increased. When concentrated nitric acid was used, the action was violent; with strong sulphuric acid there was scarcely any; and this was in strict conformity to the opinions of Fabroni; for strong sulphuric acid does not act upon zinc: but when the acid was diluted, the action was very energetic. According with this view also, solutions of sulphurets were totally inactive, although they conduct at least as well as water. From these he concluded that oxygen is necessary to the excitation of galvanism, but doubted whether or not water be, although he inclined to the affirmative opinion. Mr. Davy proved by experiment, what

had been already supposed by Nicholson, that the water suffers decomposition as well between the plates as when interposed between the conducting wires. Mr. Davy found that the gases could be separately obtained, even from portions of water in different vessels, provided they were connected by a conductor: and hence he drew the conclusion, that when water is decomposed, in the manner above mentioned, at least one of its elements is capable of rapidly passing in an invisible form through metals, and many organic bodies:——a hazardous conjecture at a period when the subject had been so little investigated. Amongst other facts he ascertained that in a galvanic series, charcoal, if well connected with the zinc, might be substituted for silver. Such a series, excited by solution of red sulphate of iron, acted more powerfully than an equal series of zinc and silver. He found that many of the difficultly oxydable metals might be made to act in galvanic combinations, by being connected in pairs, in the common order, with fluids capable of oxydating one only. In this manner, silver plates and gold wires with dilute nitric acid; or copper plates and silver wires, with solution of nitrate of mercury, produced galvanic effects. Concerning the action of the pile in vacuo, he now found that it was energetic, if the cloths were moistened in dilute nitric acid.*

* Nicholson's Journal, 4to. vol. 4. 275, 326, 337, 394, 527.

These ingenious researches of Davy presented several interesting results. But their chief tendency was, what should be the end of every investigation, the establishment of a general principle: and it must be allowed that they afforded very able support to the opinion of Fabroni concerning the chemical origin of Galvanism.

The decomposition of water, in which the component gases were found by Nicholson and Cruickshank to be separately evolved, had been presented by Davy under very singular circumstances: the phenomenon was a subject of astonishment to philosophers, and no rational explanation could be given. A suggestion was now offered by an anonymous correspondent in Nicholson's Journal,* which, with a little alteration, was afterwards adopted by Mr. Davy; namely, that the water is decomposed at each wire, that hydrogen is evolved at the silver end, that the oxygen there liberated passes to the zinc end; that oxygen is evolved at the zinc end, and that its hydrogen passes to the silver end. Mr. Davy's alteration was that the currents of oxygen and hydrogen do not cross each other in the middle, but unite and recompose water.

The same subject occupied the attention of Ritter of Jena at nearly the same time; but he drew very different conclusions. Struck with the separate evolution of the gases, he made

* Nicholson's Journal, 4to. vol. 4, 472.

some curious experiments. He procured two glass tubes, through each of which passed a gold wire sealed in the glass; the ends of the wire projecting a little within the tubes. These placed vertically, with the sealed end downward, were filled, a little higher than the wire, with strong sulphuric acid, and over this, in each, was poured a stratum of water, which was made to communicate with the pile by means of conducting wires that dipped into each tube. Oxygen was evolved in one, and hydrogen in the other. He varied his experiment, using but one tube, connected first with the positive end, and afterwards with the negative end of the pile : in the former case oxygen was given out without hydrogen ; in the latter, hydrogen without oxygen. These experiments he considered irreconcileable to the opinion of chemists concerning the constitution of water: he concluded that it is an elementary body ; that when positively electrified, it constitutes oxygen, when negatively, hydrogen, and that water may be resolved entirely into either of these gases.*

To these opinions of Ritter, the anonymous correspondent above mentioned added, that when oxygen and hydrogen are burnt, the different electricities which constituted these gases, by uniting, form fire ;—an opinion which Mr. Davy afterwards adopted, and in effect still maintains. The electrochemical hypothesis of affinity seems also to have originated in these views.

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* Nicholson's Journal, 4to. vol. 4.

Ritter appears to have hazarded this conjecture without much reflection; for, unless to the one case of water, his supposition does not apply, and not well even to that. Would he have supposed that oxygen and azote are nitrous acid in the positive and negative states of electricity? Would he have supposed that the air which we breath is nitrous acid? And what grounds were there for supposing that electricity ever entered into permanent combinations? Indeed, at this period, galvanic hypotheses appear to have been not a little inconsistent. It was supposed, by one party, that electricity is the galvanic agent, and it could be inferred evidently from their other opinions that it is both a cause and an effect in the same phenomenon. On the other hand, those who maintained the chemical hypothesis were more cautious, and merely supposed, in general, that galvanic effects are always connected with chemical action; they therefore concluded the latter to be the cause. Fabroni considered only the external exciting cause in the galvanic combination of metals; Humboldt endeavoured to explain the internal excited effect in the animal acted on. He conceived, as we have seen, that galvanic irritation and contractions are but the effect of a chemical stimulus, and that chemical agents exert the power of increasing or diminishing irritability. But both Fabroni and Humboldt, by too minute an application of the general principle, brought upon their views too much the air of hypothesis. Pfaff now

called in question the opinions of the latter philosopher. He denied the chemical action of bodies on the fibre, and maintained that these bodies act, not by increasing irritability, but by becoming new and exceedingly active links of the galvanic chain.* In cases where Humboldt had employed metals, Pfaff is no doubt right; but there are cases in which no metals were used, as in the experiment of occasioning contractions in a frog's legs by immersion into oxymuriatic acid, after they had been rendered insensible to metallic irritation by opium. Would Pfaff in this case suppose that any galvanic chain was formed, when all the parts were connected and entirely enveloped in good conductors. In the early part of his inquiries, Pfaff concluded the galvanic influence to be a fluid *sui generis*, and not identical with electricity. He afterwards altered his opinion, and subscribed to the hypothesis of Volta.

Indeed the doctrine of the identity of the two agents was as yet far from being established, and had still many opponents: it seems to have been much more prevalent in England than on the continent. The names of Fourcroy, Vauquelin, and Thenard, are found among those who considered the galvanic influence as a peculiar fluid. Robertson read to the National Institute a memoir in which he not only denied the identity of the two fluids, but conceived the galvanic to be a

* Annal. de Chim. tome 34. 307.

peculiar acid. The great dissimilarity of the shock given by the two agents, he considered a fact strongly corroborating the former opinion. That the galvanic fluid is an acid, is apparent from its taste, from its power of reddening vegetable blues, from its power of oxydating metals, and of forming a salt which he called "*galvanade*," no doubt a combination of galvanic acid with the metal.

He also described an instrument the principle of which has since been frequently used for measuring the decomposing energy of any galvanic series. It consists of a tube of glass filled with water, and containing a wire at each end which comes very near the other within. The tube stands vertically, and is graduated at its upper end, so that the water is resolved into gases, the quantity of which being ascertained by the scale, gives when compared with the time, the energy of the series.* Various other instruments bearing this name have been since contrived; but they are really electrometers, and do not measure the decomposing power of the pile. Of this kind is that invented by Mr. Pepys; it is an exceedingly delicate test of the presence of small quantities of electricity.

1801. Robertson was, I believe, the first who supposed Galvanism to be a peculiar acid which forms combinations; yet the supposition is gene-

* Annal. de Chim. tome 37.

rally attributed to Brugnatelli: but the latter does not seem to have declared such an opinion until some time after. Brugnatelli however proceeded much farther: he affirmed that in the decomposition of water, the galvanic acid becomes oxygenated. In this state it will attack and dissolve the silver conducting wire, forming a salt in dodecahedral crystals, which he denominated electrate of silver. In the same manner, he formed the electrates of copper, zinc, iron, and silver. All these he stated to be insoluble in water; but soluble with effervescence in nitric acid; the solution being precipitated by alkalies.*

These opinions, singular as they now appear, were perhaps fully warranted by the point of view under which Brugnatelli considered the galvanic agent. He observed that this power excites a phosphoric smell, an acid taste; that it inflames the skin, and excites pain in a wound like an acid; that it reddens vegetable blues, and dissolves metals. What more could any other acid do?

Although Brugnatelli and Robertson agreed in the opinion that the galvanic agent is acid, yet the former considered it as identical with electricity, and the latter supposed it to be a fluid *sui generis*; an opinion which prevailed as much in France as the other did in Italy. Lehot undertook to prove not only the circulation of a very subtile fluid in the galvanic chain, but also the direction of its motion, which he considered it

* Phil. Mag. vol. 9. 181.

possible to determine, *a priori*, by the aid of some general rules.* Gautherot allowed electricity to be present in galvanic phenomena, but conceived that it must be combined with some other agent which produces all the effects. The electricity he supposed to result from the decomposition of the water. When water changes its state and becomes gaseous, electricity is always evolved; to this cause, therefore, he attributed the production of that agent in the pile. Coinciding with his opinion, he found that there are substances which conduct electricity, but oppose the passage of the galvanic fluid; under such circumstances he found flame.† It is to be observed, however, that this is contradictory to the assertion of Ritter, who stated flame to conduct the effects of the pile perfectly. The reason of this difference of opinion will hereafter appear.

In Britain, as we have seen, the Voltaic hypothesis was generally received; but it had already begun to undergo some modifications. In this hypothesis, the oxydation of the plates was looked on as secondary: Fabroni considered it primary: Mr. Davy concluded the same thing, but upon much stronger evidence. Dr. Wollaston was the first who endeavoured to reconcile the contending hypotheses of Volta and Fabroni, and to reduce the supposed agent in both, to the observance of

* Annal. de Chim. tome 38.

† Ibid.

one general law. His opinions are comprised in the three following propositions:

1. That the influence in the pile, which produces galvanic phenomena, is identical with the electric fluid.

2. That this electricity is generated by the oxydation of one of the metals.

3. That the production of common electricity is owing to the same cause, namely, the oxydation of the amalgam.

In support of the opinion that oxydation is the producing cause of electricity, he adduced the following considerations:

Expt. 1. If a piece of zinc and a piece of silver have each an end immersed in the same vessel containing dilute sulphuric or muriatic acid, the zinc is dissolved and gives out hydrogen, the silver produces no change. But if the metals be made to communicate, even by a third metal, the silver also gives out hydrogen.

2. If the experiment be made on gold, with zinc, iron, or copper, in dilute nitric acid, nitrous gas is formed, as hydrogen was in the preceding.

3. If a solution of copper be substituted, iron will precipitate it; silver will not; but when they are made to touch, the latter gets a coat of copper.

In explanation of these experiments, Dr. Wollaston observed, it is necessary to advert to the decomposition of water by the pile. If the power be sufficient to oxydize one wire, the other affords hydrogen: and if hydrogen in this case depend

on electricity, it is probable that, in other cases, electricity may be also requisite for its conversion into gas. It would appear that in the solution of a metal, electricity is evolved during the action of the acid, and that the formation of hydrogen, even in this case, depends on a transition of electricity between the fluid and metal. By the first experiment, it appears that zinc *per se* has the power of decomposing the water of the dilute acid, and there is no reason to suppose that the contact of silver produces any new power, but that it serves merely as a conductor of electricity, and thus occasions the formation of hydrogen. In 3d experiment, iron *per se* precipitated the copper by means, no doubt, of electricity evolved during its solution, and the silver, by conducting that electricity, acquired the same power. This explanation, he observed, is confirmed by experiments with common electricity: the same transfer of chemical power, and reversion of affinity takes place. A thin silver wire was coated with wax, and cut in the middle; each cut end was immersed in solution of sulphate of copper, the other ends being connected with the circuit of a Nairne's machine. After 100 turns, the negative wire was found coated with copper; and this was re-dissolved, and transferred to the opposite wire, when the latter was made negative. Gold wires, in solution of corrosive sublimate, offered analagous results.

The identity of common and galvanic electricity, thus shown, was further confirmed by the de-

composition of water, which Dr. Wollaston effected by electrifying very fine gold points immersed in water, with a continuous stream of electricity. But in this case, both oxygen and hydrogen were evolved by each wire. It is necessary to state that this decomposition had already been accomplished, in a manner not very dissimilar, by Van Marum, Trootswick, and Deiman.

To prove that common electricity also is produced in consequence of oxydation, he tried the exciting power of different amalgama; and found the power in proportion to the oxydability of the amalgam. That of silver or platina did not act; that of tin acted well; that of zinc better; but that of tin and zinc best of all. In the same manner, a small cylinder, mounted in a vessel of carbonic acid gas, could not be excited. With regard to this latter fact, there is reason to believe that this ingenious and acute philosopher was misled by some circumstance which had been overlooked.* On the whole, Dr. Wollaston supposed, as Nicholson had already done, that elec-

* I once saw this experiment made in the laboratory of the Dublin Society. When the cylinder was turned within the receiver, in common air, it produced electric appearances. When the apparatus was put on the pump plate, exhausted, and filled with carbonic acid, it produced none, although the experiment was made opposite a large fire. When the air was let in, and the apparatus let to remain some time, I found water condensed upon the side of the receiver, farthest from the fire. The case was plain. The maker formed the frame of the cylinder of unbaked wood; the moisture was volatilized in the vacuum, and acted as a conductor, although before the exhaustion, the air of the receiver remained dry. Some such cause might have operated in Dr. Wollaston's experiment.

tricity, great in quantity but at a low intensity, constitutes what has been called Galvanism.*

In this paper, we find an arrangement of facts under a general law; and an attempt to reconcile the opposing hypotheses of Volta and Fabroni. With the former, he supposed that electricity is the agent, and he grounded his opinion upon stronger evidence than was ever brought forward on this question: with the latter, he conceived that oxydation is the cause of its production. To this, however, a fatal objection arose out of Volta's simple experiment, where no chemical action is manifested.† Some experiments shortly after detailed by Mr. Davy, showed that the preceding view required extension; for he found that other chemical changes, beside oxydation, produced the effect. He also announced a number of, what he conceived to be, newly discovered galvanic combinations, in which one metal only was employed; and these facts were looked upon as subversive of the hypothesis of Volta, which supposed that the electricity of the pile was produced by the contact of the metals.

Mr. Davy was led, by many considerations, to suppose that the alternation of two metallic

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* Phil. Trans. 1801.

† The conductor of the Annales de Chimie, in giving an outline of Dr. Wollaston's paper, observes, "Je présume qu'il abandonnera cette opinion quand il connaitra la théorié que le celebre Volta donne des propriétés de la pile." Tome 40. 170.

bodies with fluids is essential to the production of accumulated galvanism, only so far as it furnishes two conducting surfaces of different degrees of oxydability: and that this production would take place if single metals could be connected together by different fluids, so as that one surface only should undergo oxydation; the arrangement being regular. Experiment soon showed that this was really the case.

The first and most feeble class of these combinations, Mr. Davy found to be formed when a metal was, at one surface, in contact with a fluid capable of oxydating it, and at the other, with one which did not possess that power. The second class is formed when a metal is, at one surface, acted on by a sulphuret, while the other is in contact with water. The third and most active class is formed when a metal is, at each surface, undergoing a chemical change of a different nature; as the action of a sulphuret at one side, and of an oxydating fluid at the other.*

In the same manner, Mr. Davy found that other conductors beside the metallic, when in contact at different parts with different fluids, produce galvanism. Thus charcoal in contact, at one end, with nitric or sulphuric acid, and at the other with water or sulphuret of potash, if the series amounted to 20, produced shocks, decomposed water, and convulsed the limbs of a frog.†

* Phil. Trans. 1801.

† Nicholson's Journal, 8vo. vol. i. 144.

In none of these arrangements, is any principle involved which was not known to Volta, and accounted for in his hypothesis, as may be seen by examining his letters to Gren. In the case of charcoal, we have seen that Volta arranges it amongst his conductors of the first class, which, when between two different conductors of the second, will produce galvanic appearances. Volta soon after took notice of these supposed new discoveries: he observed that, so far from opposing, they are perfectly conformable to his views.*

By the generality of philosophers it had been for some time, considered as a thing demonstrated, that electricity really acts in the pile; but there was also a necessity for admitting the operation of chemical action. Inquirers, however, of high reputation, entirely rejected the agency of the former, and supposed some peculiar fluid to be elicited in consequence of the chemical changes taking place in the series. Some very interesting experiments, made by Fourcroy, Vauquelin, and Thenard, not only led them to this conclusion, but put them in possession of new facts and laws of the pile of Volta. These philosophers, with a pile of eight pairs of zinc and copper plates, and cloths moistened with solution of sal-ammoniac, heated an iron wire red hot, and caused it to inflame in oxygen. In hydrogen, and carbonic acid gas,

* Annal. de Chim. Tome 42. 157. See also Biot Tome 47.

the wire was luminous but did not burn. With a pile of an equal number of plates, of much smaller surface, the shock and decomposing power remained the same; but it did not ignite any wire. When each of the large plates was cut into four, a pile possessing the power of giving four times the shock was formed; but this was also inefficacious in igniting. Thus it was found, that the decomposing power and shock increase with the number; but that the igniting power increases with the surface. They found that the pile, which acted so powerfully on the iron wire, was not capable of affecting the electrometer. Hence Fourcroy observed that, the more experiments are multiplied, the more the pretended identity of galvanism and electricity disappears.* This opinion he seems shortly after to have altered: he admitted the identity; but, in galvanic phenomena, conceived that the electric fluid observes laws different from those which direct its motions through inanimate matter.†

That the two powers are independent of each other, was also concluded by Helwig, Twast, Bourguet, Hermann, and Grapengiesser, on account of finding that while the pile was decomposing water, its action was not interfered with by pouring in powerful streams of common electricity. During their experiments, they

* Phil. Mag. vol. 10. 93. Annal. de Chim. 39. 103.

† Système des Connais. Chim. Tome 5. 23.

effected the combustion of inflammables, as phosphorus, sulphur, gunpowder, ether, and of a mixture of oxygen with hydrogen, by means of the galvanic spark.*

A number of experiments on combustion, effected by galvanism, were made about this period. Tromsdorf discovered the combustion of metals in common air, with a pile of 180 pairs. He found that gold leaf burnt with a crackling noise and great splendour, silver leaf green, brass reddish brown, copper emerald green, zinc whitish blue, tin reddish white. In all these cases, the metals were completely oxydized; and experiment proved that by means of the spark these oxides could be deoxydated.† He afterwards found that the same dissipation of the metals, accompanied by the emission of light and heat, could be produced in the non-respirable gases.

Professor Simon of Berlin also made experiments on these combustions. He found gold to burn with a whitish yellow flame; silver with a green, edged with orange; tin sparkled with great brilliancy, darting out beautiful red rays, in the middle of which appeared a bright blue star; lead burnt with a violet flame, darting red rays; copper bluish white; zinc whitish blue; antimony burnt like tin, but with a more yellow tinge; arsenic burnt whitish blue; molybdenum burnt feebly with a red

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* Nich. Journal, 4to. vol. 4. Annal. de Chim. 41. 314.

† Annal. de Chim. Tome 40, 110. and Nicholson, vol. 4

flame. He burnt iron, gold, silver, tin, and lead, in oxygen; the flame was nearly of the same colour as before, but much more intense. The other metals could not be permanently inflamed. The combustion of most of the metals was accompanied by a decrepitating noise. He took the spark from iron wires, in the atmosphere, and observed an emanation of red rays surrounding a brilliant central light. The latter he considered as the real effect of galvanism, and the former as owing to the combustion of ferruginous particles; for in a vacuum, the central light only appeared, and in proportion as air entered, the red rays appeared and became stronger.*

The combustion of iron wire was shown, in a new and beautiful manner, by Van Marum and Pfaff. When a cup of mercury was connected with one end of the pile, and an iron wire, proceeding from the other, was applied to the surface, this end of the wire burnt beautifully, emitting rays like a sun, which could be maintained by lowering the wire as it consumed. With a pile of 200 pairs of five inch plates, they fused an iron wire 28 inches long, and ignited a wire ten inches longer. Even the refractory metal platina was fused when drawn out into a fine point. Conformably to the original results of Fourcroy, Vauquelin, and The-

* Annal. de Chim. Tome 42. 1. The experiments were made a year later; for the sake of order they are inserted here.

nard, they found that a pile of large surface will affect an electrometer, and charge a battery, to a no greater extent than a pile of small surface, provided the number of alternations are the same; yet the pile of large surface ignited an incomparably greater extent of wire. A pile of 20 ten inch plates, and one of the same number $1\frac{1}{2}$ inch diameter, were also found to give equal shocks. These effects were all produced in a greater degree when the pile was carefully insulated.

Professor Pfaff had at this time become a convert to the opinions of Volta; and many of the experiments made conjointly by him and Van Marum tended to confirm the views of that philosopher. The charging of a Leyden phial which, to a certain extent, had been accomplished by Cruickshank, was by these gentlemen performed on an extensive scale, even with large batteries. They first tried the power of 40 pairs of plates on Bennet's electrometer; and after making the battery a part of the circuit, they found it charged so as to cause the same degree of divergence. A proportional divergence was produced accordingly as the battery was charged by 60, 80, or 100 pairs of plates. These different numbers communicated to the battery the power of giving shocks, which had but one half the intensity of the number of plates that afforded the charge. They found that the pile charged 25 jars, containing $137\frac{1}{2}$ square feet of coated surface, to the same intensity as one or more jars separately:

and when the jars were charged to the same intensity by the common machine, they gave shocks precisely similar. Some experiments were also made to ascertain whether the oxydation of the metals is connected with increase of power. Muriate of ammonia, they affirm, is more powerful than even nitric acid; ammonia less powerful: strong solution of potash produced much less effect than water: yet in the latter case the polish of the plates was not even impaired.*

Than this latter statement, nothing could be more favourable to the views of Volta, and nothing more opposite to the views of almost every other philosopher; in as much as it tended to prove that oxydation is not necessary to the production of Galvanism. The contrary of this, as we have seen, was inferred by Mr. Davy and Dr. Wollaston, from a numerous catalogue of instances. This catalogue was now further extended by the researches of several inquirers. Biot and Cuvier made experiments on the subject, and from these they concluded 1st, that the galvanic apparatus decomposes atmospheric air, and absorbs oxygen; 2d, that the oxygen thus absorbed contributes to the galvanic effects; and 3d, that the galvanic apparatus has an action independent of the external air.† The matter of these three propositions had been long before

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* Annal. de Chim. tome 40. 289.

† Ibid. tome 39.

given by Fabroni; but they were now better established.

The same absorption was observed by Mr. Pepys. This gentleman found that it is greater in pure oxygen; that the energy of the pile is proportioned to the absorption, and that it produces no effect in azote or in hydrogen.*

The oxydation of the plates was also shown at this period by Desormes, who even ascertained the increase of weight which they acquired. He combated an opinion then becoming prevalent, and which was, I believe, founded on some conjectures of Ritter, Gautherot, and others; namely, that the electricity manifested in the pile is owing to the decomposition of water: for, as we have seen, Ritter supposed oxygen and hydrogen to be nothing else than water in different states of electricity; and Gautherot conceived the electricity to result from the change of state which the water underwent. Desormes also combated the opinion, first conceived by Nicholson, that the galvanic reduction of the metals, from their salts, is effected by hydrogen derived from the water of the solution: for he considered that the salt is more easily decomposed than water. In submitting water to decomposition by Galvanism, he obtained results which were afterwards the cause of much interest, surprise, conjecture, and error, throughout England, France, Italy, and Germa-

* Phil. Mag. vol. 10. 58.

ny. He found that, during the decomposition, both acid and alkali were produced; the former he judged to be muriatic, the latter ammonia.* The same result was shortly after announced by Professor Simon of Berlin.†

The necessity of oxydation, or of some other chemical change, to the production of galvanic electricity, being now generally admitted, the hypothesis of Volta, in its original state, appeared under a doubtful point of view, and evidently required modification. The view proposed by Dr. Wollaston was now (1802) new modelled, and greatly extended by Dr. Bostock; and he applied it to the explanation of particular facts, as well as to the mode of action in general. He required the three following postulates:

1st. Electricity is always generated when an oxydable substance unites itself to oxygen.

2d. Electricity has a strong attraction to hydrogen.

3d. When electricity, in passing along a chain of conductors, leaves an oxydable substance, in order to be conveyed through water, it unites itself to hydrogen, and from this it is again disengaged, when it returns to the oxydable conductor.

The first he considered as almost proved by the experiments of Fabroni, Davy, and Wollaston. As to the claims of the second and third,

* Annal. de Chim. tome 37. 284.

† Ibid. tome 41. 100.

he acknowledged them to be founded entirely on the ease with which they explain phenomena. The action of the pile, according to him, is as follows. The current of electricity, evolved during the action of the interposed fluid on the zinc plates, passes out through the positive end. If water be placed in the circuit of the conducting wires, oxygen is disengaged from it, on account of the attraction of electricity to hydrogen, and its incapacity to pass through water without being united to that gas. The electricity, thus combined, is carried to the other conducting wire, where entering the oxydable conductor, the hydrogen is disengaged. If a solution of a metallic salt had been in the circuit, in place of water, the hydrogen, instead of being evolved, would reduce the oxide; and the revived metal would collect itself round the wire. The decomposition is therefore effected at the zinc point alone, but the gases appear at both points; and the same holds even when the water is separated into two portions, provided they be connected by a conductor not oxydable.

In the construction of a pile, there are two things essential to its action. First, that the electricity be evolved; second, that it be confined and carried forward in one direction, so as to be concentrated in the end of the apparatus. The first object is attained by the oxydation of the zinc or other metal. If both sides of the zinc were oxydated, electricity would be liberated, but dispersed. When electricity is liberated, it is

attracted by the hydrogen, which is necessarily generated in the fluid that oxydates the metal; and then both are conveyed, across the water, to the silver plate; or if the pile consist of one metal only, to the other surface of the oxydating plate. By repetition of this process, electricity is accumulated.*

Such is an outline of the hypothesis as modelled by the ingenious Dr. Bostock. It was well adapted to the explanation of many facts then known; but subsequent researches have afforded a number of others, of which it would be unreasonable to expect an explanation, from principles formed at so early a period. Between the theory given by Dr. Bostock of the galvanic decomposition of water, and that proposed by Fourcroy, Vauquelin, and Thenard, there seems to be a close resemblance. These philosophers, as has been stated, supposed Galvanism to be a peculiar fluid. When this fluid passes through water, it decomposes it; the oxygen is evolved: but the galvanic fluid, by uniting to hydrogen, forms a liquid which is decomposed when it meets any wire proceeding from the negative end of the pile. There the galvanic fluid enters, and leaves the hydrogen, which is therefore suffered to escape in bubbles.

Another modification of the medium hypothesis was given by Mr. Cuthbertson, well known as an experienced electrician. This gentleman supposed

* Nicholson's Journal, vol. 3. 3.

that when zinc and copper are brought into contact, the change on their natural electricities is as great as the mutual action of the metals will allow. If to the opposite surfaces, a menstruum be applied, capable of producing a change of the metallic property, a change in their electrical property must follow. But as the change of metallic property is only superficial, the change of electric property will be effected on the surface alone. The other parts of the metals will remain unaltered, and must continue to resist. The new electric state, induced upon the surface, is opposite to the state of the parts on which no chemical action is exerted. The part of the zinc, thus acted on, must tend to throw off its electric fluid, and would give it off to that part of the copper, which by a like action is disposed to absorb it, and the two states of the surfaces chemically affected would annihilate each other, if this were not prevented by the resistance of the metallic parts not acted on. Hence the electricity is propelled from the zinc to the next copper, through the menstruum; and this being but an imperfect conductor, the propulsion is progressive.*

With regard to the supposition that chemical action alters the electric property, or what is the same thing, alters the capacity of the metal for electricity, it must be observed that this was con-

* Nicholson's Journal, vol. 2. 281.

ceived in Italy by Vassali-Eandi, Giulio, Rossi, and others. The idea was no doubt derived from the theory of electricity produced by evaporation.

Notwithstanding the various attempts that were thus making to reconcile the chemical and electrical hypotheses, and the numerous experiments which were brought forward to prove the necessity of so doing, Volta continued to maintain and disseminate his own less complicated doctrine. He had now examined the production of electricity, by contact, under all its circumstances, and even the exact intensity, as indicated by his condensing electrometer.* A commission, appointed by the National Institute, was charged to examine into his opinions, to witness his experiments, and to draw up a statement of the result. This they accomplished; and in a masterly report, they gave a methodical and succinct account of his whole hypothesis.† But as, for particular reasons, his views will be fully stated in another part of this essay, they may be here omitted.

The most deficient part of Volta's hypothesis was his position that the menstrua interposed between the plates acted better only as being better conductors. This was irreconcileable to many known facts, but especially to one which Mr. Davy opposed to it. Mr. Davy found that *very dilute* nitric acid acted far more powerfully than

* Nicholson's Journal, vol. 1.

† Memoires de l'Institut National des Sciences et Arts, tome 5. 195.

concentrated solution of carbonate of potash, a fact, as he observed, entirely to be attributed to its chemical agency on the metal, since the alkali is a much better conductor. But here Mr. Davy seems to have overlooked a material consideration. In appreciating the effects of these fluid conductors, we must take into account the difference of electromotive power which they may possess.* For the apparent electricity is equal to the absolute electricity of the plates, *minus* the counteracting electricity of the interposed fluid. The conducting power of fluids, Mr. Davy found principally to influence their own decomposition, when made a part of the series. Thus acids and saline solutions afforded gas much more rapidly, when they were decomposed by a battery of large plates, although the effect on water was the same as with small plates. Pieces of charcoal terminating the conducting wires of a battery of large plates, when brought in contact in oil, ether, and alcohol, which are bad conductors, became ignited; but in sulphuric or nitric acids, which are good conductors, sparks only were produced. Hence Mr. Davy concluded, what was then becoming a prevailing opinion, that the quantity of electricity from large plates is much greater than that from small ones; and that it passes more easily through the more perfect conductors, while by imperfect conductors its circulation is impeded.

* Murray's System, vol. 1. 634.

Mr. Davy confirmed the results of the French chemists concerning the heating power of large surfaces. With a trough of 20 pairs of 13 inch plates, he heated two feet of wire $\frac{1}{80}$th inch thick, so that it kept water at the boiling point. Four inches $\frac{1}{170}$th inch thick continued red hot for more than a minute; three inches were made white hot: and two were completely fused.*

In all the combined galvanic arrangements hitherto constructed, the contact of fluids with either metal or charcoal was considered necessary. Some experiments now made showed that this was not indispensable; and that galvanic effects could be produced by the contact of fluids with different kinds of animal matter. Galvani had indeed long before observed that feeble muscular contractions could be obtained by bringing the sciatic nerve of a frog in contact with its crural muscles. Aldini now witnessed a similar result by applying his finger, moistened with solution of salt, to the spinal marrow of an ox, and touching the crural nerves of a prepared frog, held in his other hand, to the cervical muscles about the tongue of the ox: contractions followed.†

But it is to Lagrave that we owe the knowledge of a combined galvanic arrangement, formed by different kinds of animal matter. He constructed a pile of alternate layers of muscle and brains, with discs cut from a hat, moistened in solution

* Nicholson's Journal, vol. 3. 135.

† Ibid. 298.

of salt. When 60 series were erected, a distinct galvanic taste was produced, by applying the conducting wires to the tongue.*

Although this was a pile of novel and singular construction, yet it involved no principle which was not known to Volta. In a paper read before the National Institute, Volta very distinctly described the principle of such a series as that of Lagrave, and applied it to the explanation of the electric organ in the torpedo.

Those who maintained the chemical hypothesis, and who supposed oxydation the only cause which excites electricity in the pile, must have found it difficult to conceive what oxydation could have taken place in a series of animal matter. And those who considered the interposed fluid as a mere conductor, by which the electric state of one disc was transferred to another, must be surprised that the saline fluids natural to the animal matter, and therefore interposed between and in every part of the animal discs, did not effect the same transfer and annihilation of power as would happen in a Voltaic pile, if moist cloths were interposed between the single plates, instead of between the pairs.

Notwithstanding this singular developement of electricity, the pile of Lagrave does not seem to have been regarded with that attention which it merited. The hypothesis of Volta was still re-

* Nicholson's Journal, vol. 5. 60.

tained by many, in its original state. Biot indeed, although he virtually admitted the hypothesis, rejected some of the considerations on which it was grounded. The fact which led Volta to suppose that the superior efficacy of saline fluids, for giving shocks, depends only on their superior conducting power, was his having found the intensity of a series the same, when pure water was used, as when salt was added. Biot stated, not only that he obtained a different result, but that a different result is more consonant to Volta's own views. The instruments, employed by Volta to ascertain the intensity, were considered by Biot to be inadequate ; he therefore adopted the electrical balance as preferable. He found that a pile in which the interposed fluid was solution of carbonate of soda, gave an intensity equal as we shall say to 1 : another, in which solution of alum was used, gave 1 . 58. Hence, he concluded that piles alike in every thing but the fluid, afford, in equal times, different quantities of electricity : and he conceived that any other conclusion is repugnant to the principles of Volta ; for if the conducting power of the fluids be different, the quantity conducted cannot be equal in equal times. This greater efficacy of menstrua, he convinced himself, does not depend on their greater chemical effect on the metal. For by trying the electricity of piles, the same as to number and absolute surface, although the oxydating surface was made to be of different area in each pile, he ascertained that, in no case, the extent

chemically acted on caused any difference in the electric intensity. On the whole, he concluded that the different intensity produced by different menstrua depends on their conducting power modified by the chemical changes which they undergo during the process.*

1803. About this period, a number of extraordinary discoveries were announced on the continent; some of which, if established, would have laid the foundation of a happy generalization of certain acting forces of nature. Professor Jordan of Vienna had affirmed that if water be made a part of the circuit between the poles of a magnet, it is decomposed as readily as if by the pile or electric machine. This singular experiment had not however succeeded in the hands of others: Mr. Nicholson attempted it with a five bar magnet, but without success. Nevertheless Ritter prosecuted the subject, and he has made still more singular statements than his predecessor; but I do not find that they have been taken more notice of. He affirmed that he excited convulsive motions in frogs, by means of two wires; one magnetic, and the other not. In this, however, there is nothing unexpected, for the wires without being magnetized, or one of them, would do the same. When magnetic iron wires were immersed in dilute nitric acid, the south pole was more oxydated: and when the north

* Annal. de Chim. tome 47. 1.

pole of a magnet was immersed in one portion of water, a common wire in another, and the south pole of a second magnet in a third portion, the last deposited oxide first. From these experiments, he concluded that the south pole has more affinity for oxygen than the north. He attempted to construct a battery of 120 magnets, but failed in increasing their chemical effects.* Amongst other extraordinary facts, which it would be to no purpose to mention, he stated that if two platina wires be placed, for a few minutes, between the extremities of a pile, the wires, when removed, will be capable of producing contractions in a frog. In order to discover whether common electricity would do the same, Van Marum passed the electric stream, from a very powerful machine, through two platina wires, and found that these became capable of producing contractions.† Ritter is also said to have communicated the power of causing contractions to a Louis d'or, by placing it, for a short time, in the circuit of the pile. On much the same principle, he constructed what he called a secondary pile: it is composed of one metal only, with discs of wet pasteboard. This pile does not put electricity in motion, but it may be charged by connection with an active pile. When thus charged, it possesses all the common properties, although in a weaker degree; and it loses its electricity very slowly. In these experi-

* Nicholson's Journal, vol. 8. 184.

† Ibid. 212.

ments, Ritter seems to have mistaken the principle of action. Volta shortly after showed that the common pile transmits no charge to the secondary pile: that the effects of the latter depend on the separation of the interposed water into two different strata; one acid, through which electricity flows from the metal; the other alkaline, by which it enters. He observed that it is merely a pile of his second order, namely, of one metal and two fluids.*

Van Marum found that a momentary contact of a jar with a pile was sufficient to afford a charge of maximum intensity. Ritter affirmed that Van Marum's statement is correct when the discs are moist; but as they become dry, the time required for charging is proportionately longer: and when glass was the interposed substance, no less than 12 hours were required. These statements accord with the experiments of Bouvier of Joidoigne, who found that by interposing discs of dry ice, no effect could be produced. In this he agrees with Humboldt and Fowler, although Volta maintained the opposite opinion. A battery of 128 pairs of zinc and silver, which gave violent shocks to the naked fingers, gave none when the fingers were armed with bits of ice. Notwithstanding these experiments, Mr. Dyckhoff stated that the place of wet media might be supplied by thin strata of air. A pile of 10 pairs

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* Nicholson's Journal, vol. 7.

of zinc and copper, separated by slips of very thin green glass, produced on the condensing electrometer an effect equal to five common pairs. This result was not obtained by Dr. Wilkinson, even with double Mr. Dyckhoff's number of plates. The discordance of some of these statements evidently proceeded from the presence of moisture in the ice, or from some friction which it had undergone.

With regard to the effects of the pile on the organs of motion and sensation, Ritter made some extraordinary statements. The positive pole expands the muscles; the negative contracts them: the former produces a sensation of heat, the latter of cold: one makes objects red, large, and distinct; the other blue, small, and confused. Ritter, by experiments on his body, brought on himself many dangerous fits of illness. Having made himself the circuit of communication to a pile of 100 pairs of plates during an hour, the arm on the copper side lost the power of motion, that on the zinc side seemed to acquire greater facility of motion. After this he got a severe diarrhoea, and the same is said to have happened to others who made the experiment.*

Indeed the possibility of receiving injury from the application of the galvanic stimulus to the human body has been often suggested, and many cases have been related. At the present period

* Annal. de Chim. tome 41. 209.

of our history, Larcher, Daubencourt, and Zanetti, made several experiments on the animal fluids, and they have presented us with the following conclusions:

1. Urine, acted on by Galvanism, affords a precipitate composed of the salts contained in the urine. On this account they conceive the application of Galvanism, in the region of the bladder, to be unsafe; as it is possible that the salts may be collected on its coats, and form calculus.

2. Galvanism effects a precipitation in bile: small portions of resinous, alkaline, and albuminous matter being separated.*

The beneficial effects of Galvanism in the cure of diseases have been also frequently asserted: and numerous instances are reported by various authors. That Galvanism or electricity ever effected a cure, in any complaint, is however very doubtful. It is not surprising, if during the application of so singular an agent, the person labouring under disease were restored to health, that the credit of the cure should be attributed to the means employed. In this manner, probably, Galvanism may have acquired a reputation without deserving it. An account of these medical cases may be found in Wilkinson's Elements of Galvanism published at this period, a work which contains much information on the subjects which it embraces.

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* Nich. Journ. vol. 6. 62.

1804. Dr. Wilkinson's other labours are numerous; by his frequent efforts to remove error, he has contributed much to the improvement and interest of Galvanism. Amongst his investigations, it will be necessary here to state the result of those in which he made the effects of batteries of large plates the subject of calculation. Fourcroy, Vauquelin, and Thenard concluded that the effect on animals is in proportion to the number of plates, without regard to their surface; and that the effect on metals is in proportion to the surface of the plates. The experiments of Dr. Wilkinson led him to suppose that the powers increase in a greater ratio than that of the surfaces; and that the igniting power increases, in batteries of the same total surface, as the squares of the surfaces of the elementary plates singly taken in each. This statement he afterwards satisfied himself is correct only when the surfaces are equal; but when they are unequal, the effects are as the sixth power of the diameters, or as the cubes of the respective surfaces.

Ritter, who made a number of experiments on an extensive scale, relating to this part of the subject, found that there is a certain maximum observed with regard to the different effects of a galvanic series, beyond which they diminish if the number of plates be encreased. The following summary of the result of these experiments, I have taken from the intelligent author of a well known system of chemistry. " A certain law is

observed with regard to the relation of surface and number in a galvanic arrangement, in producing the galvanic phenomena. The power of acting on living matter is most dependent on number, so that increasing the number adds to this effect; that of producing combustion is dependent principally on surface, so that increasing the number without increasing the surface is soon attended with a diminution of energy. The power of producing chemical decompositions is, in its relation to number and surface, intermediate between these. But in all of them, a certain relation exists, or a certain proportion between number and surface gives the maximum, and increasing either indefinitely, is accompanied by a diminution of power.*"

Such is the career of discovery in this new branch of knowledge, during the second period of its history; in which is comprised a space of time no greater than four years. The labours of inquirers seem now to have taken a different turn; the phenomena, in which the discovery of Galvanism originated, no longer attracted attention: researches into the economy of living bodies were found to be fruitless; the attention of philosophers was therefore directed to ascertain the laws under which inorganic matter is affected by the new agent. To this attempt we owe our knowledge of numerous facts, which otherwise might have for ever escaped discovery.

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* Murray's System, vol. I. 620.

PERIOD IV.

Containing generalizations of the chemical effects of Galvanism; and the discoveries that have resulted from the application of a general principle.

Anno 1804. THAT period of the History of Galvanism, during which so many brilliant chemical discoveries were effected by means of the new power, now commences. Philosophers seemed to vie with each other in their ardency of research, in the variety and ingenuity of their experiments, and in the exemplary perseverance with which they prosecuted their investigations.

Italy was signalized by the discovery of Galvanism; Britain by the discovery of its chemical effects; and Sweden not less by having generalized them. Hisinger and Berzelius, from the facts that had been already ascertained, as well as from a number of original experiments, drew up the following general conclusions relating to galvanic decompositions.

1. When electricity is transmitted through a fluid, the principles of that fluid are separated in such a manner that some are collected round the positive pole, and others round the negative.

2. Those bodies, that are collected round either pole, have amongst themselves a certain analogy. Combustibles, alkalies, and earths, pass to the negative side: oxygen, oxides, and acids, to the positive. Thus, water is decomposed into oxygen

at the positive side, and hydrogen at the negative: sulphuric acid into sulphur at the negative side, and oxygen at the positive.

These philosophers also effected decompositions of salts during which acids and alkalies were transferred to a distance, and through each other without combining.*

Concerning the decomposition of water, they merely gave a statement of the fact. The evolution of its constituents, at such a considerable distance from each other, had been long a subject of surprise. Mr. Cruickshank, as we have seen, invented the hypothesis of the oxygenation and deoxygenation of the galvanic influence; the latter uniting to oxygen at the negative end, and therefore liberating hydrogen; and on entering the positive end, liberating the oxygen with which it had been combined. This was presuming one end only of the pile to be active; yet as we shall see, the outline was not obliterated in some of the succeeding hypotheses.

1805. Grotthius, reasoning on this question, observed that the elementary combinations in the pile of Volta possess polarity: they may therefore establish a polarity in the elements of water. If, at the moment of the generation of these two gases, a separation of their natural electricity take place, so that the oxygen becomes negative, and the hydrogen positive, it must follow, that

* Annal. de Chim. tome 51. 167.

the negative wire will attract the hydrogen, by rejecting the oxygen; while the positive attracts the oxygen, by rejecting the hydrogen. The oxygen, on account of its negative state, may be repelled from the negative pole, and the particles of hydrogen, under the opposite circumstances, from the positive: so that there may be a series of recompositions of the fluid, those particles at the polar wires only being decomposed.

In the reduction of metals at the negative pole, no gas is liberated; whence Grotthius concluded that either the hydrogen combines with the oxygen in the oxide, or that the oxide alone is acted on; but the latter he conceived is more probable. However, even in some of his own experiments, the fact is certain; for from solution of nitrate of manganese, the hydrogen, in place of reducing the metal, was evolved. That the reduction of the metal does not depend on hydrogen had been before maintained by Desormes.

In general, Grotthius observed, the action of electricity on fluids is the attraction of the oxygenating principle to the positive pole, and of the oxygenated principle to the negative pole. If the elements be variable in their proportions, the fluid becomes oxygenated at one side, and deoxygenated at the other.*

Relating to this decomposition of water, some

* Annal. de Chim. tome 58. 54. I have, however, rightly fixed the date; for the original memoir was printed at Rome in 1805.

discoveries were about this period announced, which seemed materially to interfere with the received doctrines of chemistry. The production of acid and alkali, when water is subjected to galvanic action, had been noticed by all who made the experiment. Cruickshank considered the acid as nitrous; Desormes and Simon believed it to be muriatic; and Robertson and Brugnatelli supposed the agent, which produces galvanic phenomena, to be an acid capable of entering into combinations.

Mr. Peele, struck with the uncertainty and diversity of opinion which prevailed concerning the nature of this acid and alkaline matter, made the experiment on a larger and more satisfactory scale. He decomposed a pint of distilled water to one half: the remainder, by evaporation, afforded a salt, which in his first trial appeared to be muriate of soda, but in another, muriate of potash. Mr. Peele, with a becoming caution, merely gave a statement of facts, without anticipating the revolution in chemistry which must follow a confirmation of his experiments.*

In a very short time after, Professor Pacchioni of Pisa, made public the result of some experiments in which he had been engaged much about the same time as Mr. Peele. He observed that when water is decomposed by means of gold conducting wires, oxygen is continually emitted, and

* Phil. Mag. vol. 21, 279.

the water becomes acid: a smell of oxymuriatic acid is rendered perceptible; the gold corrodes, and an orange solution is obtained which tinges the skin of a rose colour. This liquid afforded, with nitrate of silver, a precipitate which proved to be muriate of silver: it also gave further indications of a volatile acid like muriatic, by forming a white vapour, when brought near a vessel of ammonia. From all these facts, he inferred that muriatic acid was produced by the abstraction of oxygen from water, and drew up the following conclusions:

1. Muriatic acid is an oxide of hydrogen.

2. In oxymuriatic acid, and *à fortiori* in muriatic acid, there is a much less proportion of oxygen than in water.

3. Hence hydrogen is susceptible of different degrees of oxydation.*

The grounds of these conclusions are by no means evident. I think there was equal cause to consider muriatic acid as water *minus* hydrogen; for this gas is evolved as well as oxygen. It must be confessed, however, that the opinion received some countenance from the experiments of Girtanner made long before, in which both oxygen and hydrogen were obtained from muriatic acid gas, and which he considered as proceeding from its decomposition.

It were to no purpose to enumerate the various

* See all the British Journals for 1805, and Annal. de Chim. tome 55.

ingenious persons who repeated Peele's and Pacchioni's experiments, with similar results: subsequent researches proved that substances contained in the water, in the vessels, or in the connecting media, had been the real origin of these appearances. The Galvanic Society of Paris, by using proper precautions, convinced themselves that the acid must have been derived from the animal or vegetable substances present; for when these sources of fallacy were avoided, they obtained nothing but oxygen and hydrogen.* It must be confessed that in many cases it was difficult to discover the source of the acid and alkali: but the limited production of these bodies, notwithstanding the continuance of the process, is a strong argument against the supposition of their generation: and the researches of Pfaff, Biot, Thenard, and Davy, can scarcely fail to convince that Galvanism had as yet thrown no light on the constitution of muriatic acid.

1806. More fortunate attempts were, however, shortly after, made to elucidate some subjects of chemical controversy of a different nature. The well known phenomena of metallic arborizations were long considered as involved in obscurity; and many attempts had been made to explain them, but without success. In the arborization of the newly revived metal, the latter appears, in the progress of the experiment, at such a distance from the metal to which

* Annal. de Chim. tome *56.* *152.*

the acid and oxygen are transferred, that it is difficult to understand how the transfer is accomplished. Mr. Sylvester, by an ingenious and happy application of galvanic principles, offered an explanation of the phenomenon which in the main seems to possess a great share of probability. He first stated, that if a thin coating of dissolved nitrate of silver be laid over a glass plane, with a zinc wire placed on it, an arborization ensues; the ramifications of silver shoot out a considerable distance from the zinc, which he observed is a clear proof that the reduction is not effected by the zinc. He covered one half of a glass plane with dissolved nitrate of silver, and the other half with dilute muriatic acid: one end of a platina wire was placed on the nitrate, while the other end lay on the table; a zinc wire was laid, in a similar manner, over the dilute acid. When a communication was established between the wires, a beautiful silver tree sprung up from the platina; and the effect ceased when the communication was broken. In this experiment, he considered that water is decomposed; the oxygen uniting to the zinc, and the hydrogen, conveyed to the platina, reducing the silver. In the case of the zinc wire laid on the nitrate, the first effect of the zinc is to reduce the silver in contact with it; a galvanic combination is immediately formed, which continues the process.* We shall soon find these views amplified.

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* Nicholson's Journal, vol. 14: 94.

The facts and generalizations stated by Hisinger, Berzelius, and Grotthius, were now extended and illustrated by Mr. Davy. It were a neglect of the labours of this philosopher not to declare, that scarcely any department of the new branch of knowledge has come under his examination, without being enlarged by new discoveries, illustrated by the diffusion of new light upon what was known, or impressed with some mark of his characteristic ingenuity. Coinciding with the statements of the above philosophers, Mr. Davy found that various substances, as sulphates of lime, strontites, or barytes, fluate of lime, and saline matter, even although contained in solid combinations of earths, as in basalt, zeolite, lepidolite, and lava, when placed in the circuit of the battery, suffered decomposition; their principles arranging themselves in the manner before described. He effected the transfer, through each other, of substances having strong mutual affinities, as had been before accomplished by Hisinger and Berzelius. Even insoluble bodies were insensibly conveyed, although in some instances, the force of aggregation prevailed over that of electricity. In his general conclusions he did not differ from those of Hisinger and Berzelius; except that oxydated bodies which they stated as attracted to the positive pole, he referred to the negative: and that in the passage of bodies to the polar wires, he supposed repulsion as well as attraction to operate.

Mr. Davy succeeded in effecting decompositions of saline bodies with common electricity, by the method formerly employed by Dr. Wollaston; and although, in the latter philosopher's experiment on the decomposition of water, the two gases were evolved together, Mr. Davy, by peculiar management, obtained them separately; thus more completely, as he conceived, establishing the identity of galvanism and electricity. In explaining the separate evolution of the gases from water, he gave a statement which does not differ much from that of Grotthius, and which had indeed been offered long before by some of the earlier inquirers in Britain.

Mr. Davy's next experiments seem to be the foundations on which he afterwards intended to erect his favourite hypothesis. The electricity manifested in bodies, after contact and separation, had been fully established by the experiments of Bennet, Cavallo, and Volta. Mr. Davy added to these facts a number of others. He found that when an acid or an alkali formed, with metal, an element of the pile, the former imparted to, and the latter received electricity from the metal. Although the proof of these conclusions has no other foundation than the language made use of to express it, yet as the alkali, with regard to the metal, appears positive, and the acid negative, these bodies, Mr. Davy conceived, may be governed by the common laws of electricity, when they are acted on by other electrified bodies. Thus phospho-

ric, oxalic, succinic, benzoic, or boracic acids, when touched by insulated metal, and separated, appear negative, and the metal positive; while soda, strontites, or magnesia, become positive, and the metal negative. Sulphur, separated from contact with metal, is positive; and in the galvanic decomposition of sulphuric acid, a corresponding result is obtained, the sulphur separating at the negative side. Oxygen and hydrogen are probably, he observed, in different states with regard to metals; for solutions of sulphuretted hydrogen, in galvanic combinations of single plates, act like alkalies, and hydro-sulphurets are still more powerful; so, oxymuriatic acid possesses greater energy than more highly concentrated muriatic acid.

Mr. Davy then concluded that bodies, having differently electrical states with regard to a third, must be also in different states with regard to each other; a conclusion which he found confirmed in the case of oxalic acid and lime. By analogy, he also inferred that other acids and alkalies possess similar electric relations.

In galvanic decompositions, these bodies, naturally possessed of affinity, can neither combine, nor remain in combination, if placed in a state of electricity different from that which is natural to them: thus acids separate from alkalies and oxides, oxygen from hydrogen; and metals made negative do not combine with oxygen.

On all these facts Mr. Davy founded his hy-

pothesis, that electric and chemical attractions are identical powers, and essential properties of matter. As an illustration of electro-chemical union, he instanced Beccaria's glass plates, which, when oppositely charged, cohere strongly; and when separated, still retain their charges. To make the analogy stronger, he *assumed* that different particles of matter, in combining, must still be supposed to preserve their peculiar states of energy. This doctrine he applied to the explanation of phenomena as follows.

The more perfectly the electrical energies are balanced, the more intense is the combination formed. But different bodies have different degrees of energy in relation to another body, as different acids and alkalies have to the same metal. Such bodies may be in similar or different states, with regard to each other; which last seems to be the condition of sulphur and alkalies, which have the same kind of energy with regard to metals. When two bodies, repellent of each other, act on another body with different degrees of the same electric energy, the combination would be determined by the degree, and the feeblest energy would be repelled, as in the case of elective attractions. Or there may be such a balance of forces as would produce a triple compound. Many feeble energies may overcome a few that are stronger; hence the influence of masses as shewn by Berthollet.

When bodies, differently electrified, restore

the equilibrium, heat and light result: such also is the case in intense chemical action. In chemical combinations, heat operates by giving freedom of motion, and by exalting the electric states. Thus when sulphur and copper are heated in contact, Mr. Davy found that, on separation, they shewed differently electric states, and these increased with the degree of heat: at the fusing point, there is a copious emission of heat and light. The circumstances may be similar with oxygen and hydrogen, and perhaps in all cases of combustion. If the electric states were energetic and balanced, the combination should be quick, the heat and light intense, and the resulting compound neutral: but if one state be energetic, and the other feeble, the effects are less vivid, and the compound formed ought to manifest its excess of electrical energy.

The same principles Mr. Davy applied to account for the phenomena of galvanism. He has in this manner endeavoured to connect the opposite hypotheses of Volta and Fabroni; an attempt which had been already made by Doctors Wollaston and Bostock, although in a somewhat different manner.

Mr. Davy supposed that the electric action of the metals, with regard to each other, or to the fluids of the series, is the cause which disturbs the equilibrium: that by the chemical changes it is restored; and that the consequent phenomena are produced by the joint agency of these two causes. Thus in the pile of zinc,

copper, and solution of muriate of soda, while in its condition of electrical tension, the communicating plates of copper and zinc are indifferently electrical states, and to such low intensities, water is an insulating substance. Every copper plate produces, by induction, an increase of positive electricity on the opposite zinc plate: the intensity increasing with the number, the quantity with the extent of the series. When a conductor connects the two ends of the pile, the opposite electricities tend to annihilate each other; and if the fluid medium were incapable of decomposition, the equilibrium would be restored, and the motion of the electricity would cease. But muriate of soda being composed of two series of elements in opposite states, the oxygen and acid are attracted to the zinc, the hydrogen and alkali to the copper. The balance of power is but momentary, solution of zinc is formed, and hydrogen evolved. The energy of the metals is therefore again exerted, enfeebled only by the opposing energy of the soda in contact with the copper; and while the chemical changes can be carried on, the electro-motion continues. Such are Mr. Davy's ideas on the excitation of galvanism.*

The importance of galvanism as a chemical agent being fully ascertained, and generally ac-

* Phil. Trans. 1807.

knowledged, it became more than ever desirable to gain acquaintauce with the circumstances of its production, and to discover its differences and analogies with regard to other acting energies of nature. Aldini and Lagrave, as already stated, discovered that galvanism could be produced by the arrangement of different kinds of animal matter. Dr. Baconio of Milan conceived that he had constructed a pile with different kinds of vegetable matter, impregnated with a saline fluid. By using a leaf of cochlearia as a conductor, he affirmed that contractions were produced in a frog.* It is however to be observed, that Dr. Baconio was not the first that conceived this experiment; Rossi had already constructed a vegetable arrangement, and he also conceived that it produced effects. The same naturalist, along with Giulio and Vassali-Eandi, asserted that vegetables were not only active in exciting, but even sensible to its stimulus; they are said to have produced contractions in the *mimosa pudica* and *sensitiva*. The Galvanic Society of Paris, on repeating Baconio's experiment, although with every attention, found such a pile to be totally inactive.†

The Galvanic Society also repeated an experiment of M. Marchaux, in which a pile, constructed of metals and dry paper, occasioned a divergence in the electrometer. When

* Annal. de Chim. tome 62. 212.

† Ibid. Tome 57. 61.

discs of card, dried in an oven, were used, a still greater divergence was produced. The results as described by M. Marchaux were obtained by the Society.*

These experiments, when rightly considered, must tend greatly to render doubtful the influence attributed by Volta to the interposed fluid. Volta allows the fluid to act as a conductor, and the better it conducts, the greater are the effects. In the pile of Marchaux, it was proved that the more the conducting power was destroyed, the greater were the electrical appearances produced.

Still stronger objections to the hypothesis of Volta, and even to one of its most necessary principles, arose from the experiments of Mr. Wilson. The main proposition of Volta, concerning the disturbance of the electric equilibrium by the contact of metals, was considered a simple statement of a fact, and therefore out of doubt. This, the ingenious and well devised experiments of Mr. Wilson threatened to subvert: out of a great number, it will be here necessary to state that one only which appears to be of most force. An insulated card was faced with zinc filings by means of a solution of gum; and from this, a quantity of zinc filings were poured on a bright plate of copper placed on the electrometer. Thus every particle

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* Annal. de Chim. tome 57.

of the filings were brought in contact with the copper, yet there was no evolution of electricity; and this could not have happened, were contact the cause of the electric appearances i Volta's metallic plates. When the same filings were sifted through a copper plate pierced with holes, and received on the same copper plate, still on the electrometer, the electricity was so strong as to cause the gold leaves to strike the sides: hence Mr. Wilson concluded that the separation of the zinc filings from the copper sieve produced divergence; and therefore that there are no grounds for supposing, with Volta, the agency of contact.*

Other discoveries were announced, by the persevering and extraordinary Ritter, which threatened not only the total overthrow of Volta's hypothesis, but also of every other, and even of the opinions which had been formed of some other forces of nature. Ritter, prosecuting his investigations concerning the chemical agency of magnets extended all his former views, and hazarded the following conclusions.

1. Every magnet resembles the united pair of galvanic plates.
2. Like these plates, one pole is in a different state of electricity from the other: in an iron magnet, the north pole is negative, the south positive: in a steel magnet, the reverse is true.

* Nicholson's Journal, vol. 11. 110. Mr. Wilson's experiments were made a year before: for convenience I have placed them here.

3. A number of magnets, like a number of plates, produce effects on the electrometer.

4. A battery of magnets produces the phenomena of the pile on animal matter. He concluded by observing that the earth may be an immense magnet or pile; and that hence we may account for the chemical changes which it has undergone, and for various meteorological phenomena.

I know not how to account for the silence that prevails regarding these astonishing facts, unless it be that they have not succeeded in the hands of others.*

1807. Thus it appears that, according to Ritter, the agent in galvanism, magnetism, and electricity, is the same; and, coinciding with this view, he stated, that if a needle half zinc and half silver be suspended on a point, it takes the direction of the magnetic meridian; the zinc facing the north, the silver the south: and that the two ends are feebly attracted and repelled by the two poles of a magnet. He concluded that during contact of two electromotors of the first class, the bodies are magnetic; and that, by separation, the magnetic state is destroyed, and they become electrical.†

The hypothesis which referred galvanic effects to an electrical cause, had been opposed by

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* Nicholson's Journal, vol. 13. 78.

† Annal. de Chim. tome 64.

some of the most distinguished inquirers into this branch of knowledge, on the grounds that galvanism and electricity are not conducted by the same substances. With the intention of examining the truth of these objections, Erhman of Berlin undertook a series of experiments, which terminated in some discoveries of an unexpected nature, and in the removal of the anomaly. It would appear that Erhman's attention was directed principally to the subversion of Humboldt's grounds of objection; for, between the experiments of both, there seems to be a resemblance. Humboldt found that flame of various kinds, as of sulphur and amber, vapours of different kinds, and certain solid bodies, as dried soap, &c. are conductors of electricity but not of galvanism; and Gautherot afterwards maintained nearly the same thing. Erhman showed that any substance, applied as a conducting medium between the poles of the pile, will produce one or other of the five following effects.

1. The substance, not acting separately on either of the poles, leaves them insulated.
2. Or, the poles are allowed to exert so intimate an action that they neutralize each other.
3. Or, the body permits reciprocal action, and completes the circuit; but so imperfectly that each pole continues to manifest itself.
4. Or, the body acts as a perfect conductor

to either pole; but when applied to both poles at once, belongs exclusively to the positive pole. The bodies of this class do not close the circuit, as they insulate the negative effect.

5. Or inversely, the body discharges the negative exclusively, and insulates the positive pole.

Bodies of the first, second, and third classes, namely non-conductors, perfect conductors, and imperfect conductors, are sufficiently well known; it remains to specify those of the fourth and fifth.

The flame of alcohol acts as a conductor to either pole separately; but if applied to both at once, it insulates the negative. This may be shown with a pile of 150 pairs of plates, with each end of which is connected an electrometer; insulated wires, proceeding from each end of the pile, are to approach each other so near, that an insulated flame can be made to connect them. When the flame is applied, the electrometers diverge as much as if the two ends were unconnected. But let the flame communicate with the ground by a wire, the divergence of the positive electrometer ceases, while that of the negative arrives at its maximum. Thus it is plain that the flame of alcohol belongs to the positive pole as a conductor; it does not complete the circuit, and hence the reason that water is not decomposed, when flame

is interposed between the conductors. In general, Erhman stated that substances, containing carbon and hydrogen, insulate the negative pole only; while those which contain neither, either do not act so, or insulate both: the latter happens with the flame of sulphur. The same experiments may be applied to bodies of the fifth class; and of this, the only substance which Erhman could discover was very dry soap, and the flame of phosphorus. The two latter classes he denominated positive unipolar, and negative unipolar conductors.*

These singular discoveries of Erhman gave a new interest to the Voltaic hypothesis, and tended greatly to promote its reputation, by removing the objections of some of its most distinguished opponents. The author was deservedly complimented with the prize which the French emperor annually devoted to the cultivation of this branch of knowledge.

While these advances were making towards the elucidation of the theory of Volta's pile, the practical application of it, as an instrument of chemical research, was more profitably cultivated by Mr. Davy. It is now that we find this philosopher at the zenith of his reputation; and whoever examines the extent of his labours will think with me that the brilliancy of his discoveries was no more than the reward of acuteness, ingenuity, and perseverance.

* Annal. de Chim. tome 61, 113.

Led by observation of the fact that refractory bodies underwent decomposition with ease, when placed in the galvanic circuit, their elements arranging themselves according to the general law; Mr. Davy justly concluded that bodies which have hitherto escaped the ordinary methods of analysis, might equally yield up their constituents. His first trial was on potash. After many unsuccessful attempts, he found that, if the alkali were so moist as to become a conductor, a sufficiency of heat was generated by the action of the conducting wires to effect its fusion; and that thus it was brought into a state fit for decomposition. A piece of potash was therefore slightly moistened, and placed on platina between the wires, so as to complete the circuit. The potash immediately began to fuse; there was a violent effervescence at the positive side; and at the negative, small metallic globules resembling mercury were produced, some of which burned as soon as formed. It shortly appeared that this was the wished for substance. The phenomena exactly agreed with the general law which had been formerly observed: the potash when acted upon, had given off, at the positive side, a gas which was found to be oxygen; and at the negative, a substance which proved to be combustible. This substance, when heated in oxygen, burned brilliantly, the gas being absorbed, and a solid matter obtained, which possessed all the properties of potash. So combustible was this metal that when thrown on water or ice, oxygen was abstracted, and the metal

inflamed: the same also happened in sulphuric or nitrous acid.

The metal was found to possess so strong an attraction to oxygen that, in every fluid, it immediately became oxydated, except in newly distilled naptha. At 32° it is a brittle solid, at 50° it is a soft solid, at 100° it is completely fluid.

When soda was placed in the circuit, a metal was also obtained, which was somewhat less striking in its properties. To these metals were given the names of Potassium and Sodium.* Thus by the genius of Davy was the grand chemical problem solved, in a discovery which gave a new reputation to its author, a new appearance to chemistry, a new interest to Galvanism, and redoubled ardour to investigation.

These discoveries were effected by the energy of 100 pairs of plates; and there was little doubt that increase of number would increase the power so as to separate elements still more forcibly combined. It might therefore be presumed that there are no limits to the accumulation of this force, and that applications of it might yet be made which would surpass the effects of the most powerful ordinary affinity.

Indeed the superior efficacy of the new agent, in its feeblest degree, to that of common affinity, in its most intense state, had been already shown in the case of metallic arborizations. What

* Phil. Trans. 1808. The discoveries were made in 1807.

simple affinity, for instance, would reduce one of the more oxydable metals from its solution, unaided by caloric or any other power? Yet by the immersion of a metal it is easily accomplished; and that this is by a galvanic process has been shown by the striking experiments of Sylvester, and still more strikingly, at this period, by Grotthius, as it is now necessary to state.

Sylvester, as we have seen, supposed that the reduction of the first particle of metal is effected by chemical means, and that the process is continued by the galvanic action of the newly revived particle in contact with that which occasioned its reduction. The explanation given by Grotthius is virtually the same; but as the original conception of a thing is a difficulty which when removed leaves room for improvement, the view of Grotthius is more explicitly stated, more extensively applied, and supported by more reasonings and experiments.

Struck with the increase of the arborization at so great a distance from the precipitating metal, Grotthius asks what becomes of the oxygen at the furthermost part of the tree, and of the metal, where it yields its oxygen to the precipitant? The facts might be explained by supposing the removal of the first formed stratum from the precipitant, by means of the strata subsequently produced. But to prove that the increase of the tree is effected by superposition, and not by the continual renewal of strata, he adduced an experiment in which a tube was filled

half with solution of nitrate of copper, and half with solution of nitrate of silver, the latter floating over the former. A cylinder of copper was suspended in the nitrate of silver portion: a silver tree began to form, which gradually extended its branches towards the cupreous solution; and having reached this, the silver tree acquired branches of copper. This beautiful experiment also affords another conclusion of still greater importance; namely, that since neither the silver nor the copper could separately reduce the copper of the nitrate, the reduction must have been effected by the galvanic action of these two metals in contact.

To understand how the arborization is continued at such a distance from the precipitant, it is first necessary to explain the manner in which water is decomposed by Galvanism. When the wires are immersed, that which is positive attracts the next particle of oxygen, which thus becomes elastic and is evolved; the disunited hydrogen repels the next particle of hydrogen, as both are in a similar state of electricity, and unites to the oxygen with which the latter particle of hydrogen had been combined: this latter particle of hydrogen in its turn repels the next, and unites to its oxygen, as in the former case; and finally the hydrogen is evolved, by becoming elastic at the negative wire. In a similar manner, Grotthius explained the precipitation of one metal by another. The first particle he supposed is reduced in the manner stated by Berthollet and Vauque-

lin; but the process is continued by the action of the galvanic series formed. The precipitant becomes positive by contact with the oxygen of the water, and combines with it; the hydrogen, being positive, is repelled, and it attracts the oxygen of the oxide: the metal consequently appears in its pure form. The galvanic series is now produced, and the process continues as at first.

The separate evolution of the gases, in separate portions of water, connected by wet paper, or by two fingers, he allowed might be urged as an objection to his explanation of the decomposition of water; but he considered it of no force. In both cases, there is a fluid conducting medium; and whether this be water or animal juices, the same changes may be supposed to take place, as if the continuity were maintained throughout by water. That it is the fluids which are engaged, he proved by the fact that the dry muscular fibre does not produce the effect.*

1808. To the singular cases of the precipitation of one metal by another, ascertained by Grotthius, others still more surprising were added by Bucholz. This philosopher found that metals might be precipitated from their solutions by masses of the same metal,—an apparent anomaly, seeming to prove the greater affinity of metals than of their oxides for acids. His general method was as follows. The metallic solution was

* Annal. de Chim. Tome 63. 5.

first poured into a cylindrical glass vessel: then water, in some cases acidulated with the same acid as that contained in the metallic solution, was cautiously added so as to swim on the former fluid: lastly, a bar of the same metal as that dissolved was let down, so as to be partly in contact with the lower fluid, and partly with the upper. These experiments were made with copper, silver, lead, and zinc: and in all cases, the part of the metal immersed in the metallic solution became covered with reduced crystals of the metal that had been dissolved. Hence he concluded that all metals precipitate themselves by the electric agency of their own solutions and water. The principal conditions necessary to the production of these effects are, 1st, the metallic solution must be neutral, and incapable of being acted on by an additional quantity of metal; 2d, the immersed bar of metal must be oxydable by water, and experiment showed that when the oxydation is facilitated by the presence of acid, the effects are more fully produced.*

Although the manner of these reductions was novel and unexpected, yet the principle of action was involved in Volta's experiment of the tin basin, in Ritter's secondary pile, and in Davy's series of different fluids with one metal.

The late experiments of Davy relating to the alkaline bases, at this time, attracted universal

* Annal. de Chim. tome 66. 266.

attention, and were repeated by some of the principal philosophers of Europe. They could not fail to be struck with the importance of a substance possessed of affinities so energetic that it promised to become one of the most effectual instruments of analysis ever before discovered; not less powerful than the agent which brought it into being, and far more generally applicable. Unfortunately, by the galvanic process, such small quantities were obtained that the application of it to experiment must have been almost relinquished, but for the sagacity of the French philosophers Gay Lussac and Thenard. These gentlemen conceived that if any substance could be found possessing a sufficiently powerful affinity to oxygen, the metallic basis might be obtained. Such a substance they found in iron, when aided by a high temperature. Accordingly by causing turnings of this metal to act on fused potash, in a gun barrel heated to whiteness, their expectations were amply fulfilled by the production of potassium in large quantity, although perhaps not so chemically pure as by the original method.*

These philosophers did not, however, coincide in the opinion of Davy that the new metals are the bases of the alkalies: they rather considered them as combinations of the alkalies with hydrogen, which is produced abundantly during their formation: for they found that potassium heated

* Phil. Mag. vol. 30. These experiments were read to the Institute, March 7, 1808.

in ammoniacal gas gave off hydrogen; and they assigned reasons for believing that the hydrogen was derived from no other source than the metal. This opinion they afterwards relinquished.

Ritter also conceived that the alkaline metals are compounds of hydrogen. He grounded his opinion principally on their lightness;—an argument of no force, for we must suppose hydrogen, when condensed into a solid, to be dispossessed of its characteristic lightness. He ascertained some interesting facts relating to their production. He found that a conductor of any metal, except tellurium, produced the bases of the alkalies. When the galvanic influence was transmitted through water, by means of tellurium conductors, the metal at the positive side gave out oxygen, without tarnishing; the negative side gave out no gas, but deposited a powder which was considered an hydruret of tellurium.* The same results were afterwards obtained by Mr. Davy.

These were amongst the last chemical labours of the ingenious and extraordinary Ritter; a man from whom much might have been expected, had Nature permitted the continuance of his scrutiny into her secret operations. A premature death deprived the world of one whose constitutional singularity of opinion, ardency of research, and originality of invention, rendered him at once systematic in eccentricity, inexhaustible in discovery, and ingenious even in error.

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* Annal. de Chim. tome 66. 92.

Shortly after the new method of obtaining the alkaline metal was discovered by Gay Lussac and Thenard; it was found by Curaudau that it could be also obtained by decomposing potash with charcoal. His process was to heat the two substances together in an iron tube; the volatilized metal being suffered to condense on a cold iron wire occasionally introduced for that purpose.* Curaudau considered these metals as combinations of the alkalies with charcoal; for he found that during their combustion, carbonic acid was produced. Mr. Davy did not obtain this result; and it is easy to see that the charcoal was an impurity in the metal, derived from the manner in which it was obtained.

The earths had been supposed by various persons to be metallic oxides, and the well known experiments of Ruprecht and Tondi even led those gentlemen to assert that they had obtained their metallic bases. In the hands of the celebrated Klaproth, these earthy metals proved to be no more than phosphurets of iron, formed from the materials employed in the process: and the discovery of the real bases of earths was reserved for more fortunate inquirers.

By employing the new method of analysis, the decomposition of some of the earths was now for the first time effected by Professor Berzelius and Dr. Pontin of Stockholm. These philosophers

* Annal. de Chim. tome 66. 97.

negatively electrified mercury in contact with lime or barytes; amalgams were obtained consisting of the earthy metallic base and mercury. Unacquainted with the discoveries of the Swedish chemists, Mr. Davy, while endeavouring to obtain the bases of the earths, produced combinations of these bases with metals, and the compounds formed were made by peculiar management to reproduce the earth which had been operated on. His results however he did not consider satisfactory; and they were still less so when he attempted to obtain the earthy bases in an uncombined state. Strontites, alumina, silica, zirconia, and glucina were tried by various methods; but in no case could the metallic base be obtained pure; and in some, the decomposition could not even be proved.

Berzelius and Pontin, by the action of mercury negatively electrified in contact with solution of ammonia, obtained some extraordinary and unexpected results. The mercury, in a short time, expanded to four or five times its original bulk, and became a soft solid, which when exposed to air or water produced ammonia, at the same time liberating the mercury. Mr. Davy obtained the same result, and added this fact, that an amalgam of potassium, placed on muriate of ammonia, became still more enlarged, and presented similar appearances.*

* Phil. Trans. 1808.

The next refractory substance that was forced to undergo decomposition, by means of the new instrument of analysis which Galvanism had put into the hands of chemists, was boracic acid. This acid had been considered by Fabroni as a different form of the muriatic: by Crell it was supposed to contain carbon. Davy found that, when placed in the galvanic circuit, a dark coloured combustible matter was obtained, which, when heated, gave off acid fumes, and after combustion, left a vitreous acid. The same substance was produced by heating boracic acid with potassium. It is to Gay Lussac and Thenard that we owe the complete examination of this substance. These gentlemen were unacquainted with any part of Mr. Davy's experiments but that, by Galvanism, he obtained a dark coloured substance. They heated vitrified boracic acid with potassium, in a copper tube; the resulting olive coloured mass, when washed with water, left a greenish brown substance, which was fixed and insoluble, and by combination with oxygen afforded boracic acid. To the substance thus obtained, the French chemists gave the name of Bore.* These results were also obtained by Mr. Davy, and he ascertained some new particulars. He conceived Bore to contain a little oxygen, and supposed the real base to be a metal, for which he proposed the

* Annal. de Chim. tome 68.

name of Boracium.* The name will no doubt be applicable when the thing will be discovered.

1809. The French philosophers made similar attempts on fluoric acid: and they brought to light some new and interesting facts. With a design of obtaining pure fluoric acid gas, they heated together fluate of lime and glacial boracic acid; a gas was evolved which smoked in the air, and in every gas except that of muriatic acid. This soon proved to be not fluoric acid gas, but a combination of that substance with boracic acid. When potassium was heated in common silicated fluoric acid gas, the latter was absorbed, and a solid reddish brown mass was produced. This substance, when acted on by water, afforded but one third of the quantity of hydrogen that would be given off from potassium alone. The water was found to contain fluate of potash with excess of alkali:† but there was no evidence that the base of fluoric acid had been obtained.

The researches into the nature of fluoric acid were, much about the same time, occupying the attention of Mr. Davy, but without any knowledge of the experiment of the French.‡ His results were nearly the same.

Mr. Davy made attempts on some of the simple combustibles also, but with less success. Fused sulphur, when exposed to the action of Galvan-

* Phil. Trans. 1809. 75.
† Journal de Physique, tome 68. 95.
‡ Phil. Trans. 1809. 85.

ism, evolved a little sulphuretted hydrogen. When potassium was heated in dry sulphuretted hydrogen, the metal took fire; a substance appeared which, when acted on by muriatic acid, afforded less sulphuretted hydrogen than if the potassium had been uncombined. Hence he inferred that the sulphur contained oxygen, and that it was this which, by uniting to the potassium, prevented the latter from taking so much oxygen from the water of the muriatic acid as it otherwise would.* Gay Lussac and Thenard showed that this reasoning was founded on an error, and that the muriatic acid had dissolved a quantity of sulphuretted hydrogen which otherwise would have been evolved: hence there was no evidence that sulphur contained oxygen.† Mr. Davy made similar attempts on phosphorus, obtained similar results, and drew similar conclusions: they are liable therefore to the same objections. In his trials on carbon he found that it simply united with potassium, but saw no reason to suppose that it was decomposed.

The discoveries which had been made by the agency of Galvanism taught philosophers what was to be expected from its further application: several ingenious persons had therefore, from time to time, applied themselves to the improvement of the instrument of its propagation. Mr. Children, whose ardour in the interest of this

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* Phil. Trans. 1809. 85.

† Journal de Physique, tome 69.

branch of knowledge is well known, constructed a battery which was capable of accumulating, in a given time, a much larger quantity of galvanic electricity than had been ever before witnessed. There were 20 pairs of zinc and copper plates, each 4 feet by 2, the zinc being connected to the copper, at top, by a soldered leaden strap

These double plates were moveable in a wooden trough which contained the acid; and both surfaces of the metals were exposed to the action of the exciting fluid. Mr. Children's principal results, while ascertaining the power of this battery, were as follow:

1. 18 inches of platina wire $\frac{1}{30}$ inch diameter were in a few seconds fused.

2. 3 feet were heated to a bright red.

3. Charcoal burned with intense brilliancy.

4. Of iron wire $\frac{1}{70}$ inch diameter, 10 inches only were fused.

5. The electrometer was not affected.

6. The shock was scarcely perceptible.

He concluded from all his experiments that the absolute effect of a battery is in the compound ratio of the number and surface of the plates, the intensity being as the former, the quantity as the latter.

With a battery of 1250 pairs of four inch plates, he found, contrarily to what was ever supposed, that the spark was capable of passing through a distance of $\frac{1}{50}$ of an inch. This, Mr. Children observed, removes the commonly urged objection to the identity of electricity and Gal-

vanism. He adduced as an additional evidence of their identity the resemblance of the light of both *in vacuo*.*

It is to be observed that, without doubt, these two last facts, conjointly with many others of a similar tendency already known, proved that electricity is really present in the galvanic series. But this was a truth which at this period very few philosophers doubted. Whether electricity be a cause or an effect in galvanic phenomena, was the grand question concerning which there were various opinions. Amidst this variety, the hypothesis of Volta as modified by the British philosophers, seemed to possess the greatest share of confidence. Yet even the maintainers of this medium hypothesis manifested shades of difference in opinion which it was impossible to reconcile, because originating in circumstances which the hypothesis had not been constructed to explain. Of this kind were the facts ascertained by De Luc. This veteran philosopher, by a judicious and minute examination of the pile, discovered a number of phenomena through which the knowledge of the theory of that instrument might have been much advanced, had they met with the attention which they deserved.

1810. The principal position of M. De Luc is, that the electrical and chemical effects of the pile do not depend on the same cause. He construct-

* Phil. Trans. 1809. 32.

ed a pile in which the wet cloths were in contact with the zinc only, the silver being kept separate by a brass tripod. In this state of the pile, the electric effects were produced without the chemical. When the wet cloth was in contact with the silver only, the zinc being kept separate, no effect of any kind could be observed.

He found that a common pile of zinc, silver, and cloths moistened in pure water, afforded no shock, although it produced a decomposition in water. The latter effect he conceived to depend on the oxydation of the zinc; for with a pile of pewter and silver, which produced electric appearances, no chemical effects were obtained, because there was no oxydation. But when solution of common salt was employed with the pewter, oxydation took place; the shock, electric, and chemical effects also ensued. From these and various other experiments, M. De Luc drew the following principal conclusions:

1. Certain associations of metals will produce the circulation of the electric fluid, without occasioning the shock or chemical effects.

2. For the production of the two latter effects, a fluid must constitute a part of the pile, which is capable of oxydating the metal. Pure water produces the chemical effects without the shock; but to obtain the latter, the presence of an acid is required.

De Luc found, as Marchaux and the Galvanic Society had before, that moisture is not essential to the production of electric effects in the pile.

When the metallic pieces were separated by writing paper, in its common state, a considerable divergence in the electrometers was occasioned, yet no chemical effects were manifested. When the papers were well dried at the fire, (an experiment made by Dr. Lind,) even the electrical appearances ceased: but this statement is quite opposite to that of Marchaux who made a similar experiment; for in his trial, the electrical appearances were increased by drying the interposed card. All these facts, M. De Luc observed, prove that Mr. Davy was mistaken in his opinion concerning the dependency of the effects of the pile on the insulating property of the interposed water. The pile with paper discs, M. De Luc denominated the electric column; it was shortly after applied by Mr. Forster to the ringing of small electrical chimes which, with a little occasional interruption, continued ringing for many months; so strong and constant were the attractive and repellent powers of the electricity evolved: latterly I have not heard whether they continue. A contrivance, on a similar principle, was shortly after applied by M. De Luc to the purpose of ascertaining the absolute state of electricity of the atmosphere; and to this, he gave the name of "aerial electroscope." An electrical column which I erected three years since, composed of 1000 plates, each containing on each surface five square inches, remained in good action for about a month, its effects then diminished, and soon after ceased.

M. De Luc found that a column of small discs

produced electricity of no greater intensity than one with discs of much larger dimensions, but the time required for the effect was in the inverse ratio of the surface. He ascertained also that water, placed in the circuit between the extremes of the column, completely destroyed the divergence of the electrometer.* How some philosophers could still maintain that water is an insulater is not easy to understand.

In the progress of those investigations, which added so much new interest to chemistry, Mr. Davy observed the singular phenomenon that charcoal, intensely heated by galvanic electricity in oxymuriatic acid gas, produces no change. This gas had been supposed to hold its oxygen so loosely combined that it was surprising it did not yield it to a substance having, at this high temperature, so strong an attraction to it as charcoal. The fact led Mr. Davy to an investigation of the subject, and to the promulgation of opinions which, if established, seemed to threaten the overthrow of the whole French system of chemistry. As this important subject originated in the application of Galvanism as a chemical agent, it will be here necessary to give a sketch of these opinions, and of the controversy to which they gave origin.

Oxymuriatic acid was discovered in 1774 by Scheele, who considered it as a simple substance which when united to phlogiston formed muriatic

* Nicholson's Journal, vols. 6, 7.

acid. In the general revolution which chemistry underwent towards the close of the last century, Berthollet gave quite a different view of its nature: he showed by analysis and synthesis that it consists of muriatic acid combined with oxygen, but by a very weak affinity. The constitution of this gas was, from that time, considered as one of the best established facts in the Science. Gay Lussac and Thenard at length, endeavouring to obtain dry muriatic acid gas, hoped to do so by abstracting oxygen from oxymuriatic acid gas; for they satisfied themselves that the latter contains no combined water. They accordingly, amongst other trials, attempted to accomplish their purpose by passing oxymuriatic gas over ignited charcoal; but in this they were disappointed, for the gas was not decomposed into muriatic acid gas, unless when water or some other body containing hydrogen was present. When water is thus present, or can be produced, oxymuriatic acid gas may then be decomposed by bodies which have an attraction to oxygen: for in this case the resulting muriatic acid gas can and does combine with the water which is necessary to its constitution, and which it always contains while in the gaseous state. The impossibility of its existence without this combined water is the reason why oxymuriatic gas is not decomposed by ignited charcoal when pure: for in this case, there is neither hydrogen nor water present; and as without the latter, muriatic acid gas cannot exist, it therefore is not formed, and the oxymuriatic acid

remains unchanged. They confirmed these results by the coincidence of many others; and they observed that all of them may be explained by a supposition which some of them lead to, namely, that oxymuriatic acid gas is a simple substance, which by combining with hydrogen forms muriatic acid: but they considered that the phenomena are better explained by supposing the gas to be compound, as in the commonly received theory.*

Nevertheless, the fact above mentioned induced Mr. Davy to adopt this discarded opinion: and he commenced an experimental investigation of the subject, which finally removed all doubts from his mind of the undecompounded nature of oxymuriatic acid gas. His first inquiries tended to show that there is no evidence of the existence of oxygen in this acid gas. He heated tin in it, and obtained a compound which, according to the old theory, is composed of oxide of tin and muriatic acid. If this be true, ammonia should separate the oxide; yet when ammoniacal gas was presented to the compound, it was absorbed, a solid was obtained, and there was no separation of oxide. When oxymuriatic acid acts on phosphorus, a solid combination is formed, which according to the old opinion, must consist of muriatic and phosphoric acids: the latter of which, if really present, would prove the existence of oxy-

* Memoires d'Arcueil, tome 1. 358.

gen in the acid gas. The compound was saturated with ammonia, and it then became a dry powder, which must be muriate and phosphate of ammonia. Heat ought to expel every thing from this but phosphoric acid: yet experiment proved that even a white heat produced no change. No greater indications of oxygen could be obtained from sulphuretted muriatic acid. When oxymuriatic acid gas acts on ammonia, or on sulphuretted hydrogen, or hydrogen, it is commonly believed that water is formed: Mr. Davy could find none: and in the last case, the result was pure muriatic acid gas. Thus in all these trials there was no evidence of the existence of oxygen in oxymuriatic acid gas.

Gay Lussac and Thenard had proved that when oxygen is procured from oxymuriatic acid gas, water is always present, and muriatic acid gas is formed. Oxymuriatic acid gas, by combining with hydrogen, produces muriatic acid gas only. Hence, Mr. Davy thinks it is scarcely possible to avoid the conclusion that the oxygen is derived from the water; consequently that the existence of water in muriatic acid gas is an hypothesis which rests on another, namely, that oxygen exists in oxymuriatic acid gas. As to the water observed when oxymuriatic acid gas is passed over litharge, it can be accounted for without supposing that it existed in the gas: the latter may lose hydrogen, and the former oxygen.

If then there be no reason to suppose the presence of water in this gas, we can account for

the evolution of hydrogen, when metals act on it, in no other manner than by allowing hydrogen to be a constituent of muriatic acid: and accordingly, synthetic evidence convinced Mr. Davy that this is the case, since by the combination of hydrogen with oxymuriatic gas, the result was muriatic acid.

On this view, it may be conceived that when metals act upon muriatic acid gas, the oxymuriatic gas is taken from hydrogen, and an oxymuriate is formed. When water is added, it is decomposed, the oxygen uniting to the metallic base, and the hydrogen to the oxymuriatic acid gas, thus forming a real muriate. Potassium unites with oxymuriatic acid gas, and forms oxymuriate of that metal; if water be added, the metal becomes oxide, the oxymuriatic acid becomes muriatic: both combine and form muriate of potash. If the latter be ignited, the oxygen and hydrogen again unite, as also the metal and the oxymuriatic acid gas, as in the first instance. This example will suffice to give a general specimen of the application of this new doctrine. The oxymuriatic acid gas, Mr. Davy supposes may be a peculiar acidifying principle analogous to oxygen; and to this principle he afterwards gave the name of *chlorine*.*

1811. These opinions, as may be supposed, excited general interest, and were shortly put to

* Phil. Trans. 1810.

the test of experiment by an experienced chemist. Mr. Murray of Edinburgh conceived that the new doctrine had no advantage over the old; that every fact advanced in support of one, could be explained by the other, and even in a more satisfactory manner. The synthetic experiment of combining oxymuriatic acid gas with hydrogen, and forming muriatic acid, Mr. Murray explained, according to the old system, by supposing that the oxygen and hydrogen form water, which is not precipitated, but is dissolved by the muriatic acid as soon as formed. The same explanation may be given of these cases where oxymuriatic gas acts on sulphuretted hydrogen, or ammonia. The observation of Gay Lussac and Thenard, instanced by Mr. Davy, that when oxygen is obtained from oxymuriatic gas, water is always present, and muriatic acid formed, may, Mr. Murray conceived, be explained by supposing that the oxygen is derived from the oxymuriatic gas, but that water is necessary to aid the decomposition, by the affinity which it exerts to muriatic acid.

In the case brought forward by Mr. Davy, where metals evolve hydrogen from dry muriatic acid gas, an oxymuriate of the metal being formed, it is easy to suppose that the hydrogen resulted from the water, of which it is impossible to free muriatic acid gas by exposure to substances having a strong attraction to moisture: the attraction increases as the moisture in the gas decreases: hence the combined water will soon

be held by a stronger attraction than that which it possesses for the deliquescent body.

After showing that all known facts may be explained by the old theory, Mr. Murray observed that there are two methods of determining the question: either it may be shown that muriatic acid gas contains water; or that oxygen exists in oxymuriatic acid. The former is rendered more than probable by the known affinity of this gas to water, and by the considerations just mentioned concerning the action on it of deliquescent bodies. The latter, Mr. Murray proved by mixing one measure of carbonic oxide, one of hydrogen, and two and a half of oxymuriatic acid gas, each being previously well dried. In 24 hours, the green tinge of the latter gas had disappeared: and on admitting water, a condensation equal to more than one half took place; and the residual gas produced a milkiness in lime water, indicating the presence of carbonic acid. Thus oxygen must have been afforded to the carbonic oxide.

If oxymuriatic acid be a peculiar acidifying principle, it is, as Mr. Murray observed, an anomaly in the new doctrine that charcoal is not acidified by it, while the fact is accounted for by the old theory; for as water is necesssry to the constitution of muriatic acid, it follows that this gas cannot be liberated when there is no water present; hence the oxygen and charcoal resist the attraction that subsists between them.

Such were the principal objections made by Mr. Murray to a doctrine, which involved no

less a question than the validity of the whole French system of chemistry. Whoever examines his paper will probably allow that its object was important, that its arguments were conceived with acuteness, and expressed in that moderate language which promotes the true end of discussion, and renders controversy pleasing. Nevertheless the task of replying to these objections devolved upon Mr. J. Davy, who with much ability conducted the defence of the new doctrine.

With regard to Mr. Murray's observation, that the non-union between oxymuriatic acid and charcoal is an anomaly in the new doctrine, Mr. J. Davy stated that it must be looked on as an ultimate fact, of which an explanation is not to be expected: but he observed that the explanation given by Mr. Murray, according to the old doctrine, is hypothetical, as the necessity of water to the constitution of muriatic acid has not been proved. He even conceived that the contrary is proved; since this gas, in any degree of dryness, always affords the same quantity of hydrogen, when acted on by metals. As to the carbonic acid which Mr. Murray obtained by detonating oxymuriatic gas, hydrogen, and carbonic oxide, Mr. J. Davy denied the fact, unless oxygen, or, what is in effect the same, water be present: for when ammoniacal gas was employed to examine the residuum of the detonation, nothing but carbonic oxide was found. Mr. Murray, in his reply,

again adverted to the want of action between ignited charcoal and oxymuriatic gas, which in the new doctrine is given as an ultimate fact incapable of explanation. Mr. Murray allowed that comprehensive facts, arrived at by generalization, may be looked upon as ultimate; that the same indulgence cannot be granted to individual facts; that of such kind is the one in question; and that the old hypothesis explains it, while on the new, it is an anomaly. Concerning the presence of water in muriatic acid gas, he thought that the evolution of equal quantities of hydrogen, by the action of metals on that gas in any degree of dryness, is not a valid objection, and that the statement itself is incorrect; for Dr. Henry found that muriatic acid gas, when acted on by electricity, afforded hydrogen in quantities which varied as the gas was dry or otherwise. The formation of carbonic acid when oxymuriatic gas, hydrogen, and carbonic oxide, were allowed to act on each other, Mr. Murray found to take place, contrarily to Mr. J. Davy's assertion, even when ammoniacal gas was employed to examine the residual gas. Mr. J. Davy, on the other hand, after further examining this result, allowed that the carbonic oxide does disappear, but conceived that it combines with oxymuriatic gas, forming a gas which has not hitherto been known, and to which he afterwards gave the name of *phosgen*. He now admitted that carbonic acid appears; for the phosgen gas combines with the

ammonia added, and when this compound is acted on by a dilute acid, water is decomposed, the oxygen unites to the carbonic oxide, and the hydrogen to the oxymuriatic gas: hence result carbonic and muriatic acids.

The existence of water in muriatic acid gas, which had been objected to as hypothetical, Mr. Murray now thought necessary to confirm by direct experiment: and in summing up the state of the question he observes, "according to Mr. Davy's hypothesis this gas (muriatic) is the real acid free from water, and it is admitted that if it contains water, that hypothesis is precluded, and the common one is established."* Mr. Murray therefore combined dry ammoniacal gas with muriatic acid gas, and heated the muriate formed; water in a tolerably large quantity was expelled. Mr. Davy considered that this water was absorbed from the air during the exposure of the salt: but, in subsequent experiments, Mr. Murray not only showed that water is produced, although the salt were not exposed to air, but that muriate of ammonia absolutely never attracts moisture, even when exposed for a length of time. To this last experiment no answer has been given; and I know not to what cause Mr. Davy's silence should be attributed, unless his latter trials have offered a result similar to that of Mr. Murray.†

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* System of Chemistry, vol. 2. 648.

† Nicholson's Journal, vol. 28, &c.

Chemists of high consideration still adhere to the old theory.

The view of the nature of oxymuriatic acid given by Mr. Davy, had been claimed by Curaudau. He denied, it is true, that it contained oxygen; but his opinion is declared by a commission of the National Institute to be founded upon inaccurate experiments.*

There was also another important question, concerning which Mr. Murray maintained an opinion different from that of Mr. Davy. The metals obtained from the alkalies had been supposed by Gay Lussac and Thenard to be compounds of the latter substances with hydrogen; but this opinion they relinquished. Mr. Murray also conceived the metals of the alkalies to be compound bodies, but proposed a different view of their formation. The researches of Berthollet proved that potash, in the dryest state in which ignition can afford it, still retains a large quantity of water. This water, Mr. Murray conceived, cannot escape decomposition when the potash is exposed to galvanic agency. If so, what becomes of the hydrogen; it is not evolved, and therefore must combine with the resulting substance. On this view, potassium is to be considered as a compound of the real unknown metallic base, with hydrogen; and Mr. Murray supported his opinions by a variety of arguments and experiments, which it is un-

* Annal. de Chim.

necessary here to detail, the question being properly a subject of chemical research.*

Some other opinions of Mr. Davy were now brought under examination by the author of this Essay. Mr. Davy, as we have seen, framed an hypothesis, in which the identity of chemical and electrical attraction was assumed. I endeavoured to prove that the propositions of this hypothesis are contradictory amongst themselves, and irreconcileable to the known laws of electricity; and in this opinion, as will be shown, I have not been singular. Mr. Davy's supposition that different classes of bodies exist in differently and permanently electrical states is unsupported by any fact, and is opposed to all the knowledge of the subject which we possess. To explain the cause of permanent union by means of this evanescent power, Mr. Davy requires that different particles of matter in combining be still supposed to preserve their peculiar states of energy. This postulate, if granted, would certainly, so far, explain the phenomenon; but it ought not to be admitted without other claims than its convenience in extricating the hypothesis from a difficulty. We are even compelled to reject the supposition, since every fact indicates that absolute states, if different, can never exist in contact. But admitting combining bodies to be in different states, and these states to be retained after combina-

* Nicholson's Journal, vol. 29.

tion, these powers do not account for the phenomena of affinity. Thus in the decomposition of water, by copper wires proceeding from each end of the Voltaic series, one wire unites to oxygen; why should not the other to hydrogen: or if the wires be of platina, why do they refuse to combine with either oxygen or hydrogen, if difference of electric state be the cause of combination? I adduced an experiment in which an acid and an earth, which Mr. Davy supposed to unite by difference of electrical state, were brought into a similarly electric state of high intensity, and yet they combined as readily as if in different states. Other instances were also adduced, in which bodies in similar states combine: thus phosphorous acid and oxygen are naturally negative, yet they combine. Amongst a variety of other facts and arguments, I endeavoured to show that if, in the decomposition of compounds by galvanism, the elements were really separated by electricity, they should combine with increased force; and hence that a combination, and not a permanent decomposition, should be the invariable result.*

The subject again comes under examination in the second part of this Essay, where I shall have an opportunity of commenting on Mr. Davy's reply.

The same hypothesis also occupied the attention of Dr. Maycock. In support of the ex-

* Phil. Mag. vol. 37.

istence of naturally different states in bodies, Mr. Davy lays it down as a simple expression of facts, that hydrogen, alkalies, metals, and certain metallic oxides, are attracted by the negative, and repelled by the positive side; and that oxygen and acids are attracted by the positive, and repelled by the negative side. These opinions Dr. Maycock made experiments to prove unfounded. He took one substance out of each of the above classes, namely, boracic acid, barytes, and gold leaf; and found that they were all equally and indifferently attracted by positive or negative electricity. Hence, in these bodies there was neither preference nor aversion to either kind of electrical state; and yet this is the foundation of Mr. Davy's entire system.

Even if it were allowed, although in opposition to every analogy, that dissimilar bodies are in differently electric states, and that these states are retained after combination, Dr. Maycock was of opinion that Mr. Davy's hypothesis is far from being sufficient to explain the phenomena. If water be composed of oxygen and hydrogen, and if these bodies be negative and positive with regard to each other, it does not follow, he conceived, that they are so to every other body: the oxygen will be positive to a body that is more negative, and the hydrogen will be negative to one that is more positive. He supposed (although he has not stated upon what grounds) that repulsion is not

exerted between bodies, unless they be precisely in a similar and equal state; hence there will be only one point of positive electricity, at which the positive wire will repel hydrogen, and only one of negative at which the negative wire will repel oxygen: and at all the other points of excitement, the positive wire will attract hydrogen, and the negative wire oxygen. Therefore repulsion is rarely if ever exerted, and the decomposition must be referred to the unequal attractions of the wires; each attracting both gases, but with unequal degrees of force: and these attractions will be modified and counteracted by the attractions of the opposite wire. Thus if the positive wire attract oxygen with a force = 20, and hydrogen with a force =10; and if the negative wire attract hydrogen with a force = 20, and oxygen with a force = 10; the efficient attraction between the positive wire and oxygen would be = 10, that between the negative wire and hydrogen = 10, and the power tending to separate the gases = 20. The effect of the wires increasing with their difference of state, the hydrogen must be more positive than the positive wire, and the oxygen more negative than the negative wire. In this manner only, will it appear that as the states of the wires are augmented, their action on water must increase: for the nearer the state of the positive wire comes to that of hydrogen, and the state of the negative wire to that of oxygen,

the stronger should be the efficient attraction of the positive wire for oxygen, and of the negative for hydrogen. The same reasonings apply to all decompositions, and the constituents of all bodies must be considered as having electrical states more widely different than those of the wires of the battery. But this is shown to be impossible by Newton's experiments with the pendulum, and by every experiment with the electrometer.

Dr. Maycock then laid down the following statement of the facts, viz. that hydrogen, alkalies, and certain metallic oxides are, after their separation from oxygen and acids, found at the negative wire: and oxygen and acids, after separation from the other class, at the positive wire. He found, as indeed had been well ascertained before, that during the contact of bodies, no electrical signs are manifested; and not until after separation. In the same manner, particles of matter, while in union, are in the natural state, but acquire different states by separation. Hence this difference is the effect and not the cause of decomposition.

As to the manner in which galvanism produces decompositions, Dr. Maycock considered the operation as analogous to the effects of caloric. During the restoration of the equilibrium between electrified conductors, the latter are often fused; that is, a repulsive force is caused to be exerted between their particles. This repulsion separates the constituents of

whatever substances are placed in the circuit of the bodies discharging electricity; the constituents, by separation, acquire oppositely electrical states, and hence their appearance at the respective poles.

Dr. Maycock next observed that the discoveries of Wilcke, Æpinus, Volta, and Davy, have led by induction to the law, that contact and separation of dissimilar metals produce on them states of electricity opposite in each. He considered that every case of electric excitement is dependent on this law. Thus during the friction of the cylinder in the common electric machine, the parts of the glass which come in contact with the rubber, are immediately after separated by the revolution of the cylinder. But this idea is not the exclusive right of Dr. Maycock; for long before, it had been distinctly stated by Mr. Wilson, although the paper in which it was contained escaped attention. Mr. Wilson's words are, "I am very much inclined to believe that the excitation that takes place in friction is caused by the same circumstance (viz. contact and separation), and that the friction does nothing more towards the excitation than to bring the different parts of the substances, that are rubbed together, into contact, and separate them from it."* M. De Luc however did not consider these expla-

* Nicholson's Journal, vol. 11. 110.

nations admissible; and as to the supposition that separation is the cause of the electrical appearances, he instanced that in the pile there is nothing but contact, without separation.*

In all the galvanic arrangements hitherto described, the presence of two different metals with one fluid, or two different fluids with one metal, was considered indispensable. A series was, about this period, constructed, which apparently offered an objection to the general law, by being composed of but one metal and one fluid. It is described by its inventor, M. Sweiggeir, as composed of arcs of copper, one leg of each of which is immersed in a different vessel of dilute sulphuric acid. In an arrangement of such vessels alternately connected, the one containing a determinate leg of the copper arc, being throughout the series heated by a lamp placed underneath, the leg in the heated acid performs the part of zinc, and dissolves, while that in the cold acid remains unaltered. But in this arrangement it does not appear that any new principle is involved; it is no more than the case of a metal in contact, at one surface with a fluid capable, and at the other with a fluid incapable of oxydizing it, a series sufficiently familiar. Hot and cold sulphuric acid must be considered different fluids, inasmuch as they possess different properties.†

* Nicholson's Journal, vol. 32.

† Journal de Physique, tom. 73, 405.

The only possible mode, at present known, of causing one metal and one fluid to present galvanic phenomena, is by making such a series a part of one, in which galvanic phenomena are already going forward: in this case the single metal, at its different ends, acts the part of two different metals. This fact had been the cause of many mistakes and extravagant conjectures, until Mr. Murray of Edinburgh undertook a more minute examination of the subject, and concluded from his experiments that the case is a mere deception. Mr. Murray conceived that the arc or wire of single metal passes into an electrical state, by its proximity to the compound arc of the acting arrangement. That end of the wire opposite to the negative leg of the compound arc becomes positive, and either oxydizes or evolves oxygen; the other end becomes negative, and evolves hydrogen. Or if instead of water, a solution of a neutral salt had been employed, it is decomposed, and its elements arrange themselves according to the general law, on account of the electric states assumed by the single wires, on the principle of induction.*

1812. That period, during which the ardour of experimental investigation began to decline, is now arrived. Those whose talents and inclination led them to the cultivation of Galvanism, while

* System of Chemistry, vol. i. 605.

in its infancy, seem to have forsaken it, and to have left it to its own merits, now that it had arrived to maturity. Sir H. Davy, whose name acquired no new lustre from a title of honour, seems to have concluded his labours in this department of knowledge by the publication of his "Elements of Chemical Philosophy," in which is contained a succinct arrangement of the facts and theories of Galvanism, and also of his own hypothesis of electro-chemical affinity. The doctrines of Volta, as modified by the British philosophers, had come into much repute; and the electro-chemical hypothesis, which Sir H. Davy had interwoven with it, seems to have been adopted and modified by some philosophers of high consideration. The views of the compound hypothesis, given at this period by Berzelius and Oersted, require to be stated. The following, which is that of Berzelius, seems to differ little from that of Davy.

In nature, a series of mutations continually takes place, and the powers which occasion these changes are light, caloric, and electricity. All these have a relation, and one often produces the other. When a large pile is discharged by two platina points, a small sun is produced, surpassing every thing else in light and heat: and when this happens, the two electricities cease to manifest themselves as such. This shows a relation between caloric, light, and electricity, which time may develope.

In all our experiments, caloric and the electri-

cities seem continually to tend to an equilibrium, and to arrive at a state of repose, which is the end of all affinity. But this equilibrium is ever broken by the Sun's rays, and a continual process of disturbing it, and of keeping the powers active, is therefore carried on by that luminary. Common fire is produced either by combination of bodies, or by that of the two electricities, or by friction, or by compression. The phenomena of the Sun cannot be produced by friction or compression, as appears by every consideration: they must therefore be owing to the union of electricities, resembling the discharge of the two platina points above mentioned: and this, from the arrangement of things, must, when commenced, continue for ever.

It is well known that the electricities are much concerned in the operations of affinity, but we know not how. We know, however, that two bodies having affinity are, when brought into contact and separated, found in opposite states; that having the greater affinity for oxygen being positive, the other negative. And since the higher the temperature, the higher will be the electric intensity; and since at the moment of union, the heat often becomes intense, we may conclude that there is at this moment a discharge of electricities which produces fire. After this union, there is a state of chemical repose: and the elements cannot be again separated, without the influence of a mass of electricity in a state of disturbance, as in the pile. In this case, each

electricity acts upon its relative constituent part, to which it restores its original characters.

The manner of this change may be thus. The atoms of bodies may possess electric polarity, on the intensity of which their force of affinity depends. The affinity is thus identical with electric polarity. In any body, one pole may be more positive than the other is negative, and *vice versa:* in the former case, the body is electro-positive; in the latter, electro-negative. Heat augments the polarity: hence the different affinity of bodies at different temperatures.

The cohesion of homogeneous particles may be compared to the juxtaposition of the metallic and resinous plate of an electrophorus, while in contact, and in a state of charge or neutralization. This neutralization is destroyed when the surfaces are separated. When heterogeneous particles combine, the opposite poles touch, and the electricities discharge themselves; heat results, and the particles remain combined until by some means, the poles resume their opposite states.

As bodies combine in which the same state predominates, it appears that the force of affinity depends rather on the intensity of the general polarity, than on the specific unipolarity. Hence sulphur and oxygen, although of the same unipolarity, have greater affinity for each other than oxygen for gold, although the two latter possess opposite unipolarity.

In this case, when two bodies of the same unipolarity combine, the new particle has both forces

concentrated in one of its poles, its affinities are therefore more intense. Hence sulphuric is the strongest acid. When bodies of an opposite polarity unite, the polarity of one generally predominates, as in potash, and in oxides: but sometimes neither predominates, as in neutral compounds.

Combining atoms must turn their opposite poles to each other; hence combination is more easy when one or both are in the liquid state, and generally impossible when both are solid. And as each atom must have an electric atmosphere of determinate extent, they can only act at determinate distances: hence the ease with which some liquids combine, and the difficulty that attends some combinations of gases, especially when rarified.

The chemical action of the pile consists in the re-polarization of the particles of a combination. In a combination of similarly unipolar particles, "the pile merely restores, by the decomposition, the general polarity, because their specific unipolarity was not changed by their union; but in combinations of opposite unipolarity, it likewise restores the specific unipolarity of their elements."

As the electricities of the pile discharge each other, and produce fire; so caloric when accumulated, and tending to regain its equilibrium, may disappear and re-appear as electricity, and separate itself into two opposite states. Hence bodies are decomposed by heat; and as their elements cannot separate without acquiring difference of

electric state, the latter is derived from the caloric by its resolution into positive and negative electricity.*

Such are the views of Professor Berzelius. It is to be observed that he conceives it more consistent with facts to suppose electricity as well as caloric to be matter: and in this respect he dissents from the opinion of Davy.

The modification of the electro-chemical hypothesis proposed by M. Oersted of Berlin differs from that of Berzelius in some respects; yet beside a general resemblance, there seems to be a coincidence of particular views. This modification unites the original speculations of Davy and Ritter with the peculiar opinions of the author. It is said to have excited much interest in Germany; but Oersted's work is little known in Britain, and from the accounts given of his hypothesis, it is hard to be convinced that he has not been too bold in the assumption of his principles, or that he has been sufficiently cautious in their application.

He considers electricity, galvanism, magnetism, heat, light, and chemical affinity, as depending on the same forces; one of which is positive, the other negative. These forces are opposite and capable of destroying each other; their mutual extinction produces heat and light.

With respect to the electrical forces of bodies, those which are attracted to the same pole are simi-

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* Nicholson's Journal, vol. 34. 142.

lar. Acids and oxygen are possessed of a force opposite to that of alkalies and combustibles, these classes of bodies are therefore attracted to opposite poles. Hence bodies are divided into two classes or series, 1st, products of combustion, and 2d, supporters and combustibles. Those of one series do not combine with those of the other, except sulphur and phosphorus, which combine with both metals and alkalies, and thus constitute a kind of transition between the series. The author distributes the bodies of these series into a kind of arithmetical progression, beginning with the most combustible, proceeding to those little combustible, and ending with a body absolutely incombustible. This body, which is oxygen, possesses properties perfectly opposite to combustibility; but from its attraction to combustibles, it produces that strong action called combustion.

The series of products commences with the most energetic alkalies, proceeds to those more feeble, and arrives at bodies in which the opposite properties are balanced. The next are bodies in which acidity predominates, feebly in those nearest the point of equilibrium, but energetically in those further removed. Bodies belonging to the opposite extremes unite so forcibly as to change their state; for the order of affinities is changed in the product: thus oxygenated and certain hydrogenated combustibles are products; and an acid united to an alkali is no longer a product but a salt. Bodies nearly alike in their affinities, as two acids, remain in the same series.

All substances which have the same preponderating force are comprised within this law, and in such a manner that they cannot be confounded. The same force may exist in a state so different, that its attraction for the opposite force cannot make it enter into combination. Supporters and products possess the states most widely different: the neutral salts bear a close resemblance to those products that are placed near the point of equilibrium. Combustibility is the preponderance of the positive force in the state of supporters; alkalinity, in that of products.

Combustion affords either alkaline, acid, or neutral products. The product, by combining with oxygen, loses partly or entirely its free positive force; and the oxygen, its negative force. If the body be highly combustible, and do not combine with too much oxygen, the body is still positive and alkaline. If it be but little combustible, and combine with much oxygen, the body is still negative and acid.

In oxygenated bodies, the opposite properties, alkalinity and acidity, are united; one of those is often rendered insensible by the other, but in many cases, both subsist together, as in the oxides of lead, copper, &c. In some bodies, the positive and negative forces are found in both states at once. Thus, ammonia possesses the positive force of both the first and second classes; and the nitric and oxymuriatic acids possess the negative force of the first and second classes. This is the effect of a combination either feeble

or far from the point of saturation. In the combination of tellurium with hydrogen, the negative force of the metal is so changed by the positive force of the hydrogen as to occasion acidity: but the positive force of either is not sufficiently changed to become an acid. Hence, telluretted hydrogen is at once combustible and acid.

When the opposite electric forces unite, heat results, which will be intense as the obstacles to the transmission are great, provided they be not insurmountable. Thus chemical union excites heat; but cold may be produced if the conducting power be great. Expansion is the effect of repulsion arising from an excess of either electric force. Contraction is the effect of equilibrium and of the extinction of the forces.*

I shall not pretend to offer any commentary on this hypothesis, not having had sufficient opportunity to understand it fully. It however appears that the connexion of galvanism and chemistry is not as yet sufficiently ascertained to enable us to generalize the facts comprised by the latter according to principles derived from the former. In this way, perhaps, the doctrines in question may be considered as prematurely promulgated: and the same observation may, probably, extend to all the electro-chemical hypotheses.

1813. With regard to these views, some curious

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* Annals of Philosophy, vol. 5. 5.

experimental illustrations, relating to the attraction of certain bodies to a determinate pole of the electric circuit, were at this period brought forward by Mr. Brande.

Mr. Cuthbertson had shown that when the flame of a candle is placed in the interrupted circuit of a common electrical machine, the heat and flame are attracted principally to the negative surface. Erhman showed that there is a class of conductors which do not conduct the electricity of either pole indiscriminately, but belong to one exclusively: and to this cause Sir H. Davy attributed Mr. Cuthbertson's result. It occurred to Mr. Brande that the facts admitted of another explanation. He conceived that the determination of flame to a particular pole is no more than a new case of electro-chemical attraction, and that the fact is a strong argument in favour of the natural electric states of bodies. Thus in the above-mentioned experiment with a candle, the smoke and flame are attracted to the negative side; the flame and smoke of phosphorus are attracted to the positive surface. In the former case, he supposed that the carbon and hydrogen of the flame are attracted to the negative surface, because these bodies are naturally positive; in the latter case, that the phosphoric acid is drawn to the positive side, because that acid is naturally negative. He then tried a number of experiments on the flame of various bodies, with an apparatus in which the electrical circuit was interrupted by two brass balls, separated to a cer-

tain distance from each other, and each containing a thermometer. The flame being placed between the balls, the thermometers indicated to which side the elements of the flame were attracted: and the results all agreed with what was expected.*

The inquiries of Mr. Brande properly conclude the history of those investigations which tended to the advancement of the theory of Galvanism. It is however necessary to subjoin an account of some galvanic arrangements, very different in effects and extent, but, if rightly considered, all agreeing in pointing out the real nature of the agent which had been supposed to be electricity. Mr. Singer, the author of a work on this subject, of much merit, exemplified the effects of De Luc's electrical column on a scale never before witnessed. He arranged the immense number of 20,000 zinc and silver plates, separated by dry paper, into a column. With this, a divergence of two inches was occasioned between pith balls one-fifth of an inch diameter. Sparks were obtained from wires which were a minute distance asunder. A jar was charged so as to give a shock up to the shoulders, and even to fuse an inch of very fine platina wire. But what is most surprising in this instrument, is its total incapacity to effect the decomposition of even the most loosely combined substances.†

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* Phil. Trans. 1814. Read Nov. 1813.

† Nich. Journal, vol. 35.

Far different was the battery at this time put in action by Mr. Children. It consisted of 20 pairs of copper and zinc, each plate 6 feet in length, and 2 feet 8 inches in breadth. The pairs were connected by bands of lead; and to each pair was allotted a separate wooden trough. This battery ignited 6 feet of thick platinum wire: it melted platinum, iridium, and also the ores of iridium and osmium. In consequence of a suggestion from Dr. Wollaston, it was found that a greater length of thick than of thin platinum wire could be fused by the galvanic heat.

1815. Suspecting that the immense metallic surface exposed in this battery did not produce proportionate effects, and knowing, from some experiments made by Dr. Wollaston, that the surfaces of zinc and copper opposed to each other were alone active, Mr. Children attached two copper plates to each plate of zinc, each surface being thus opposed by copper. The effects of this arrangement were as follow.

It heated 8 feet 6 inches of platina wire, $\frac{44}{100}$ inch diameter, red hot. Oxides of tantalum, uranium, and titanium were fused, but not reduced. Oxide of cerium fused and burnt, but did not reduce. Oxide of molybdenum easily fused and reduced. Pure iridium fused imperfectly. Ruby, quartz, silex, sapphire, and plumbago did not fuse. Gadolinite and zircon fused, but the latter imperfectly. Magnesia was agglutinated. Blue spinel ran into slag.

By heating iron in the circuit in contact with

diamond powder, the iron was converted into steel: a fact that put an end to the diversity of opinion which subsisted on this question. The battery gave no charge to the Leyden phial.

Mr. Children then tried the effect of the battery on compound wires composed of two different metals joined end to end and extended in the circuit. Wires of platina and gold, gold and silver, iron and gold, platina and zinc, zinc and siver, were tried, and in all cases the first mentioned metal of each compound wire ignited, and the other did not; but the zinc in both cases melted. When lead or tin was tried with platina, neither ignited, but the two former metals fused at the point of contact. Iron ignited with zinc, but the zinc was not melted. Gold and copper both became red hot. With a wire of platina and iron, the iron first ignited at the pole; the platina then ignited entirely; this decreased, and the iron became hotter.

These results were explained by Mr. Children, on the supposition that the difference of heating power was owing to the difference of conducting power: the worst conductor being most heated, on account of offering most resistance to the transmission. If this supposition be well founded, the order of conducting power will be silver, zinc, gold, copper, iron, platina: the order of the heating power will be therefore inversely.*

* Philosoph. Trans. 1815.

The heating power of a galvanic combination was exemplified on a singularly minute scale in the elementary battery of Dr. Wollaston. This consisted of a thimble with its top cut off, flattened until its sides were $\frac{1}{5}$th inch asunder. It had but one plate of zinc, inserted and secured between these sides, with cement. This apparatus, when charged with water, acidulated by $\frac{1}{50}$ its measure of sulphuric acid, ignited a platina wire $\frac{1}{3000}$th of an inch diameter, and in length varying from $\frac{1}{30}$ to $\frac{1}{50}$. The zinc plate, in this contrivance, has both its surfaces opposed by a different metal, and to this cause Dr. Wollaston attributes its great power.*

CONCLUSION.

SUCH is the hasty, but I would venture to hope, not indistinct survey of the history of Galvanism from its origin to the present day. Whoever reflects on the shortness of the period in which this immense collection of facts has been accumulated and generalized, cannot fail to be struck with the ardour and perseverance which have distinguished the various inquirers into this recondite branch of knowledge.

* Annals of Philos. vol. 6. 200.

The discoveries effected by Galvanism are numerous, diversified, and important. The opinion long maintained, that all matter is essentially the same, has been strengthened by the new analyses of natural bodies into fewer and more simple forms of matter. New light has been thrown on the laws of affinity, both with regard to combination and decomposition. Philosophers are now enabled to establish more comprehensive generalizations: and bodies which were considered directly opposed to each other, are shown to be analogous or nearly related. Thus a new power has been acquired of bringing to light bodies possessed of the most active means of eluding observation. These in their turn are made instrumental in evolving others, which since the creation of things were wrapt in impenetrable concealment. Our knowledge of animal physiology is also extended by the researches that have been made into the economy of the organs of secretion, circulation, and motion. Through the mediation of Galvanism, the secrets of retiring Nature have been drawn from her bosom, and the industry of man is rewarded by a more enlarged survey of the stupendous revolution of changes.

But while we admire the effects produced on other branches of knowledge, by the cultivation of Galvanism, we are compelled to regret that the ardour manifested in the inquiry has been detrimental to itself. The invention of hypotheses, at too early a period, occupied the attention of those whose talents would have been so much

more advantageously exerted in experiment: and in this we have to regret, not only mispent time and labour, but also the early bias acquired by the mind, which is ever unfriendly to the reception of true impressions. It is not a little remarkable that even the first discovered phenomenon was distinguished with an appropriate hypothesis; but as might be expected from the nature of its foundation, its weakness was discovered, and it was consigned to destruction. On the ruins of this was a new one raised,—a splendid edifice, the conjoint labour of genius and industry. Here Truth lay in chains where she ought to have possessed dominion: Imagination usurped empire over Reason; and every thing has since submitted to that influence which it seemed so difficult an undertaking to subvert.

END OF THE HISTORY.

PART II.

Examination speculative and experimental of Galvanic Hypotheses.

IN the progress of the foregoing historical sketch, a statement has been given of the principal hypotheses of Galvanism which ingenious speculators have at different times devised: but a more useful, interesting, and arduous part of the undertaking remains to be attempted, namely, the examination of their truth. There are two methods of estimating the pretensions of an hypothesis; firstly, by ascertaining the agreement or disagreement of its propositions amongst themselves: secondly, by ascertaining the agreement or disagreement of its propositions with facts. The evidence of both these methods conjointly being of a more convincing nature than either separately, I shall endeavour to adduce it in the following examination. The hypotheses of Volta, and of Fabroni, shall be first considered; then the medium hypothesis adopted by the British philosophers: next Sir H. Davy's hypothesis of electro-chemical affinity; and lastly shall be discussed the question whether the agent in galvanic phenomena be electricity. These shall therefore be the subjects of consideration in the five following chapters.

CHAPTER I.

Statement and examination of the hypothesis of Volta.

IT is much to be regretted that the Italian philosopher has not collected his opinions into such a form as to constitute a connected series. His doctrines and experiments are scattered through a great variety of journals, transactions of societies, and periodical publications; and there is not a complete summary of his opinions in any one of them separately. Hence a number of misconceptions have arisen; objections have been started which the hypothesis had been previously constructed to meet, and arguments have been devised to prove opinions which had been already controverted by experiment. In the following statement, I have endeavoured to give a correct view of the hypothesis by collecting it from the writings of its author, or of those who had the best opportunity of understanding it.

Volta divides the conductors of electricity into two classes; the first comprises metals, certain minerals, and charcoal: the second contains fluids of various kinds. The former are called perfect, the latter imperfect conductors.

By the contact of two dissimilar conductors of whatever class, the electric equilibrium is dis-

turbed: but the power of producing this disturbance is different in different conductors. Two conductors thus in contact constitute the true element of the pile.

To establish a *current* of electricity, it is necessary that there be *three* conductors; either one of the first class in contact with two of the second: or one of the second class in contact with two of the first: or that there be an arrangement of three conductors of the second class. When any of these three conditions are fulfilled, the current will be established: its direction will be decided by the predominating force of some one of the three bodies: and its intensity and quantity, by the specific electromotive power of the conductors employed.

To prove the disturbance of the electric equilibrium by contact of conductors, and to show the circumstances of their disturbance, Volta assumes zinc and copper.

Expt. 1. Let an insulated plate of each metal be brought into contact, and afterwards separated: the condensing electrometer will indicate that the zinc is positive, and the copper negative.

Expt. 2. Let a plate of copper and a plate of zinc be soldered end to end; let the zinc end be held between the fingers, the copper being applied to the copper plate of the condenser; the condensing plate will be charged negatively, as will appear on withdrawing it.

Expt. 3. If the copper end be held, while the

zinc is applied to the copper plate of the condenser, there is no disturbance effected. But,

Expt. 4. If a moist conductor be interposed between the zinc end and the copper plate of the condenser, the latter becomes charged positively. And,

Expt. 5. If the copper end be applied to the moist conductor, at the same time that the zinc end is held between the fingers, the electrometer plate is charged negatively.

These experiments are explained by Volta in the following manner. In 1st and 2d the electric equilibrium is disturbed. In 3d, the electricity, as before, passes from the copper to the zinc; but the latter is then applied to the copper plate of the condenser, which instantly tends to discharge its own electricity into the zinc: hence there are two powers acting in opposite directions, and no disturbance ensues. In experiment 4th, the excess of electricity passes off from the zinc through the water of the moist conductor: for water has little or no power of moving electricity, and therefore does not occasion a current in an opposite direction. The water acts as a simple conductor to the condenser, which hence becomes positively charged. In experiment 5th, the same thing takes place conversely; the copper end, being negative, communicates its state to the condensing plate, by the conducting power of the water. These are the circumstances which relate to the *disturbance* of the equilibrium.

With regard to the conductors necessary to

produce a *current* of electricity, Volta gives the three following arrangements:

1. Two different metals and a stratum of fluid.

2. One metal in contact at one side with an acid, at the other with sulphuret of potash.

3. Muscle, nerve, and blood.

From the preceding account of the mode of action of the elements of galvanic series, it is easy to account for the action of the elements in combination, as in the common pile. In the following numerical statement, the excess of electricity in the zinc over the copper is represented by unity: the pile is supposed to be insulated, and to commence with copper.

The copper and zinc plate commencing the apparatus, on being applied to each other, disturb the equilibrium; the former becomes $-\frac{1}{2}$, the latter $+\frac{1}{2}$: over the zinc is placed the moist conductor. The next metal is copper; it acquires the state of the under zinc, and this can only happen at the expense of the first copper plate, which therefore becomes $-\frac{2}{3}$, while the zinc and the second copper plate become $-\frac{2}{3}+1=\frac{1}{3}$, and the sum of all will be equal to zero. If a second zinc plate be now placed over the second copper, the former must acquire an unit more than the latter; this must again take place at the expense of the first copper plate, which therefore is reduced to -1, while the upper zinc plate is $+1$, and the two middle plates $=0$, or they are in the natural state. Thus in every pile, the two ends must be equally but differently electrical,

and the sum of the negative and affirmative quantities must be equal to zero, and in the middle, there must be two plates in their natural state.

If a connexion be formed between the copper end and the ground, the copper assumes what it had lost; it is now in the natural state or equal to 0, the next zinc plate becomes +1, the next copper plate also 1; the next zinc plate becomes 2, and the increase is in arithmetical progression. If a finger be now applied to each end of the pile, the excess is discharged through the animal circuit, and a shock is felt: even in recently dead animal matter, a convulsion is produced. If, instead of water, the pasteboards are moistened in a saline solution, the shock is greater, because such a fluid conducts better than water: but the electric tension indicated by the electrometer remains the same. The oxydation is secondary, and merely establishes a more intimate connection between the elements of the pile.

The first principle, namely, that by the contact of two metals, the electric equilibrium is disturbed, one acquiring an excess, and the other suffering a diminution, is a position in direct contradiction to every thing that has been hitherto ascertained concerning the laws of electricity. The principle can be shown to depend on experiments which admit of a different explanation, and which have been rendered doubtful, as to their accuracy, by not succeeding in the hands of others. Lastly I shall endeavour to show that were the principle even admitted, it would be

insufficient to explain the phenomena to which it has been applied.

In every experiment with common electricity, when we intend to restore the equilibrium between two metals in different states, we bring them into contact, and we find that the effect is produced, for they no longer manifest symptoms of the electricity which they possessed. The only cause, hitherto known, sufficient to counteract the coalescence of different states, is the interposition of a perfect or imperfect non-conductor: but in the present instance, the bodies concerned are of the most perfect conducting nature. How then is it possible that the means which we use to restore the equilibrium should at the same time disturb it? Hence the principle under consideration being contradicted by the established laws of electricity, cannot be deducible from them; we must therefore seek what other claims it may have to be admitted.

It is true that zinc and copper plates, after separation, show difference of electrical state; but it does not follow that they were so while in contact: and experiments have been made which seem to prove the contrary. Dr. Maycock showed this with plates; and Mr. Wilson ascertained the same by pouring zinc filings on the bright copper plate of an electrometer; no divergence ensued, although there was thus an extensive contact: but when the filings were poured from a copper surface, they caused the

gold leaves to strike the sides, when they fell on the copper electrometer plate.* Here then it is beyond doubt that the separation of the filings, in their fall from the copper, produced electricity, and that their contact manifested none: so that the state of the plates, after separation in Volta's experiment, affords no support to the principle in question. It might indeed be supposed that his experiments with the two plates, soldered end to end, are a proof: they therefore require investigation.

As a preparatory step, it is necessary to advert to the structure of the pile; especially to that called by De Luc the electrical column, in which dry discs of paper are employed. The arrangement is copper, zinc, paper; copper, zinc, paper, and so on: every pair producing electricity in that which is placed over it.

In Volta's second experiment abovementioned, the zinc end is held; and the copper end is applied to the copper plate of the condenser: we may consider the copper end and the copper plate as one piece, because thus in contact; so that with the zinc end, a combination is formed precisely resembling the first pair of plates in the pile, but beginning with zinc. The next body connected with the copper is a coating of lac varnish with which the condensing plate is covered:

* I have repeated this experiment, and obtained the same results in a very striking degree.

and this is analogous to the disc of dry paper. Immediately in contact with the varnish, is the brass plate of the electrometer, which containing zinc in its composition, acts as such.* Attached to the brass plate are the gold leaves, which, with the brass, act as copper, but more powerfully. Thus the whole is an arrangement of two series similar to that of the pile, and might be called a series composed of zinc and copper in contact, paper (or varnish), zinc and copper in contact (or what is the same, brass and gold exercising their functions). It would not therefore be remarkable that four conductors, arranged two and two, should produce a divergence in an exceedingly sensible condensing electrometer.

But if, as in Volta's third experiment, the copper be held in the hand, the zinc being applied to the copper plate of the condenser, it will happen, even according to Volta, that the zinc being between two plates of the same metal, will attract electricity equally from both pieces of copper, and therefore will transmit none. The separation by the coat of varnish will in this case be of no avail, as it is interposed between the metals which ought to be in contact.

Thus it appears that in Volta's fundamental experiments, there is no evidence in favour of the supposition that it is *contact* which disturbs the electric equilibrium : for there is a substance, in

* The experiments of Mr. Wilson have shown that brass produces with copper a very strong positive electricity.

its effects analagous to paper, interposed between the pairs, so that a small pile acts, if there really be such an action: and we shall shortly see that even the phenomena of the pile will not bear out the conclusion that contact is the cause. It is also to be observed that Volta's experiments, receiving them as presented by him, do not prove their object, since to ascertain the presence of electricity, he absolutely separates the condensing plate from the other plate of the electrometer. It is therefore manifest that the facts admit of a different explanation; one that is accordant with known phenomena; while the opposite one is opposed by every fact within the compass of our knowledge.

Hitherto, in my observations on Volta's experiments with the condenser, I endeavoured to account for his results without supposing them connected with any fallacy; and to show that allowing them to be correct, it is possible to explain them without admitting his fundamental principle. That there was no source of mistake is extremely doubtful; or rather is is certain that the condenser, employed by Volta, is not an instrument to be depended on. It is well known to those in the habit of using this instrument that the coating of resinous varnish is apt to become charged in an unaccountable manner, and by almost imperceptible causes. It is also known that the *kind* of electricity produced in the condensing plate is different at different times, even where it

is not possible to assign a reason for the variation. With so capricious an instrument is it not incautious to draw inferences opposed to thousands of experiments which, as far as we know, are not liable to fallacy, which have been made in every country, by numberless persons, and with invariable results ?

These considerations are enough to render the principle doubtful: but there are stubborn facts that must infinitely increase the doubt. Mr. Cuthbertson, who it will be allowed had at least as good an opportunity of procuring the most improved apparatus, declares his results to be quite opposite. With a delicate condensing electrometer in which no varnish was used, Mr. Cuthbertson found that if the condensing plate were copper, touched by the zinc end of a compound bar similar to Volta's, the plate on *separation* was negative : but if the plate were zinc, touched by the copper end of the bar, the former was found in the positive state. Here then is nothing but the simple well known fact, that when zinc and copper are separated from contact, the former becomes positive, and the latter negative: and it matters not whether the bar be composed of copper soldered to zinc, or of either metal entirely.

The strongest evidence that can be produced in favour of the supposition that contact is the means of evolving electricity, is the action of the pile, in which the plates are stationary and suffer

no separation, notwithstanding that electricity is demonstrably present. Before this objection can be removed, it will be necessary to consider the following facts.

If a piece of zinc and a piece of silver be placed, one under the tongue, and the other between the upper lip and gums in the usual manner, and if the ends which project from the mouth be brought into contact, a flash will be perceived. If the contact be still continued, there will be no other flash: but one takes place the moment the metals are separated. Now since there was no flash while the contact was maintained, and since separation produced one, it is evident that there must have been a little distance between the plates when this happened: and it is likely that the first flash took place when the metals were at the same distance in their approach. To try if this were so, I coated the zinc plate with an exceedingly thin film of solution of red sealing wax in alcohol: with this plate, when dry, I repeated the experiment, and perceived a flash notwithstanding that the film prevented real contact: but this succeeded only when my mouth was well washed with dilute nitric acid. Furthermore, if the contact of two metals be made perfect by soldering the ends which should project from the mouth; and if the zinc limb of this arc be placed under the tongue, and the end of the copper limb be applied between the upper gums and lip, there will be no flash. These facts tend to prove that the electricity of metals is not evolved by con-

tact; and it appears to me that the real exciting cause of electricity in the pile is the approximation of the metals within a certain distance, which thus allows them to exert on each other some specific action. This view has several advantages; it does not suppose that the electricity of metallic plates is an anomaly, irreconcileable to the established laws of common electricity: and it does not refer the disturbance and restoration of the equilibrium to the same cause, as is unavoidable in the hypothesis of Volta. It is certain that by contact and separation of Volta's plates, they assume differently electrical states; these two plates are the element of the pile: may we not then suppose that its different states are owing to the same elementary cause? The pile is no more than a series of contacts and separations, each pair of plates being in *contact*, and *separated* from the next by some non-metallic substance. So far, the explanation just offered is on a level with that of Volta, both being apparently dependent on a principle which admits of no further investigation, and is therefore to be considered ultimate. But it will be shewn in the sequal that, while Volta's is an ultimate fact, my view can be referred to a more general principle. Agreeing with my explanation, I found that a pile of 120 zinc and copper plates, each of which was very thin, and had a slight bend in the middle, so as to occasion elasticity in the plate, afforded a certain divergence; but this was much increased by alter-

nate compression and dilatation of the spring: that is, by occasioning an alternation of contacts and separations.

It has been already observed, that it is repugnant to the laws of common electricity to suppose the evolution of electricity owing to contact. It may be objected, that although repugnant to the laws of common electricity, yet it may be a property of the electricity of metals. With such a supposition the following experiment does not coincide.

I procured a six inch circular plate of thin zinc closely perforated with very small holes, highly polished, and set in a rim, so as to resemble a sieve. Through this was sifted a quantity of bright and very fine copper filings, which were received on a large plate of polished copper placed on the electrometer: the gold leaves diverged negatively, and continued to do so for some time. Meanwhile a copper sieve, containing very fine zinc filings, was agitated gently over the electrometer plate: the gold leaves gradually collapsed, and at length came closely together. The agitation being continued, the gold leaves again diverged, but with positive electricity. Here then the different states of the metals, when presented to each other, were destroyed.

From this it is evident, that metallic electricity obeys the law of common electricity, that contact does not disturb, but restores the equilibrium, and that the cause of the electrical appearances is

separation. Nor is this an idle logomachy, nor an attempt to establish a distinction in which there is no virtual difference; it is a matter of importance to the hypothesis of Volta, for if this main proposition be denied, there are various phenomena which become inexplicable. Thus, if a piece of zinc be soldered to a piece of copper, and if both be immersed in dilute sulphuric acid, the copper gives out hydrogen abundantly, yet is not acted on. If in this strict contact, no electricity be evolved, whence arises the singular action of the copper on an acid which it otherwise would not affect? If a wire of zinc and a wire of copper be soldered end to end, bent in the form of a U, and the ends applied in a certain manner to a frog, contractions follow: this also would be then inexplicable by the hypothesis. The latter phenomenon indeed does not seem to be explained, even admitting the position in question. In the bent wire, the copper portion was incapable of containing its natural electricity, being connected with zinc: the latter therefore received it, having a greater attraction or capacity. I see no reason why this excess should be transmitted through the animal, back into the copper, which it appears was incapable of containing it, and which parted with it a moment before: and since the zinc took it from the copper, I know not why it should give it off again, having still the same power to retain it. Should it be said that a current of electricity is established by the animal conductor, we would deceive

ourselves with words: there is no sufficient reason assigned for the formation of such a current, since the causes which disturbed the equilibrium continue to act, and since no new force is excited, the animal matter being a mere passive conductor.

I have thus attempted to shew that the main proposition of Volta's hypothesis is questionable, and even opposed to numerous facts. The next thing to be considered is the influence of three plates in contact, two being of the same metal, and no imperfect conductor interposed.

According to Volta, when a plate of zinc is laid over a plate of copper, the former receives a stream of electricity from the latter: but if, over this zinc, another plate of copper be placed, it also acquires a tendency to pour a stream into the zinc, in a direction contrary to the former: hence the two efforts destroy each other, and as his experiment on the electrometer indicates, the motion of the fluid ceases, and the equilibrium is restored. In such a state, it is evident that the three plates should exercise no galvanic power: if presented to any acid menstruum, nothing should take place but the ordinary chemical action. To investigate this, I formed the abovementioned combination in three different ways. A ring was formed, half zinc wire and half copper wire, so that the zinc was in contact at each end with copper: a bar of copper was soldered to one end of a bar of zinc, and another bar of copper to the other end, thus form-

ing a componnd bar of which the middle alone was zinc : a plate of zinc was soldered by its flat surfaces, at each side, to equal plates of copper. Each of these combinations was thrown into dilute sulphuric acid, which should act on the zinc alone. The consequence was, that the zinc dissolved rapidly, and *both* pieces of copper, in each case, evolved abundance of hydrogen. The question therefore remains,——since it was not electricity, what caused the copper, which is not acted on by the acid, to evolve hydrogen? View this fact in any manner, and it is not reconcileable to the hypothesis of Volta : if, in such a combination, the electric equilibrium be not restored and at rest, why does it not affect the electrometer? if it be at rest, why have the copper pieces the power of extricating hydrogen?

Another fact of the same tendency is, that if a plate of zinc be connected with two pieces of copper, so as to be between them, and if the different metals be applied to a frog, in the usual manner, contractions will be produced as powerfully as if the zinc were in contact with copper at one surface ouly. The fact, like the former, is inexplicable, if we suppose the electricity to have been at rest; but if we suppose it to have been in a state of activity, the non-action of the metals on the electrometer, as stated by Volta, remains to be accounted for. These observations tend to evince, what will be considered more fully hereafter, that electricity has nothing to do in the phenomena.

The precise state of the question, whether or not contact be the cause of the electricity, therefore is as follows. The principle is opposed by the laws of electricity: it is founded on experiments which admit of a different explanation; which were made with a doubtful apparatus; which did not afford the same results, when instruments less liable to fallacy were employed; and which, when tried in a still less exceptionable manner, (as by Wilson's method), presented results entirely opposite. My experiments, with the metallic filings and sieves of different metals, shew that contact, instead of disturbing, restores the equilibrium: and those with zinc, in contact at each surface with copper, are shown to be inexplicable, whether we suppose electricity to be in action or at rest.

Waving all the preceding objections to the principle in question, and supposing it for a moment true, it will be advantageous to inquire how far it would explain the phenomena for which it was intended. If a plate of copper be covered by one of zinc, if over this a wet cloth be laid, and finally another plate of copper, the hypothesis supposes that the latter copper plate *must* assume the state of the zinc at the expense of the lower copper. Of this I cannot see the necessity, for it is easier to suppose it possible that the upper copper could remain in the natural state, being separated from the positive zinc by a less perfect conductor,

than to admit that one of the two metals *in contact* should be in the negative state. But even averting from this, there is no cause assigned why the upper copper plate should acquire the state of the zinc, at the expense of the lower copper. It were much more easy to conceive that the necessary quantity of electricity can be abstracted from the hand that lays down the plate, and which directly communicates with the common reservoir, than that it shonld be derived from the lower plate, which is insulated. The hypothesis seems therefore defective in not assigning adequate causes for the increasing positive state towards the top of the pile, and for the increasing negative state towards the bottom.

In fine, keeping in mind Volta's main principle, namely, that copper and zinc by contact disturb the equilibrium, the one acquiring what the other loses, it is easy to arrive at a knowledge of the state in which the pile ought to exist, deducing this state from the hypothesis itself. Let us first suppose that the metals are distributed into distinct pairs, the copper being underneath in each pair: the copper in each is $-\frac{1}{2}$, the zinc $+\frac{1}{2}$. If over one pair, another be placed, either with or without the interposition of a moist cloth, it must happen that each pair will preserve the exact state in which it existed previously to its being so placed: for it will be found that, in this case, the regular alternation of the plus and

minus state should be preserved, as there is no cause to subvert it. Let the first copper be represented by *a*, the first zinc by *b*, the second copper by *c*, and the second zinc by *d*; then *a* will be $-\frac{1}{2}$, $b+\frac{1}{2}$, *c* will be $-\frac{1}{2}$, and $d+\frac{1}{2}$; or commencing the series with the second plate, *b* will be $+\frac{1}{2}$, $c-\frac{1}{2}$, $d+\frac{1}{2}$: in either case, the regular alternation of contact between plus and minus is equally maintained, and if moist cloth be interposed, it only acts as a conductor, as the hypothesis supposes.

Hitherto, the pile has been supposed to be insulated. When the lower or copper plate is connected with the ground, the state of the whole is said to be instantly altered. The negative state is annihilated throughout; the first copper abstracts from the ground what it had lost, and therefore becomes neutral. From this plate, the positive electricity increases upwards, and reaches its maximum at the top. If a finger be applied to the top and bottom of the pile, the excess will be discharged, through the animal conductor, into the common reservoir, and will produce the sensation of a shock.

To this explanation, two principal objections may be made. First, since one half the pile, which was in a state of deficiency, could abstract from the common reservoir as much electricity as brought it to the natural state, it should follow that the other half of the pile ought equally to have parted with its redundancy, so as to assume the natural state also. To answer

that the quality of the zinc is to preserve its excess, and that the arrangement of metals is such as to urge the electric stream forward, were not obviating the objection; for we have seen that in the insulated pile, the lower zinc plates were even highly negative, and the upper copper plates equally positive. It appears therefore that in this part of the hypothesis there is an imperfection, since it fails to assign a reason why, in the uninsulated pile, the positive state should not be annihilated as well as the negative.

The other objection is as follows. It is stated that if both ends of the uninsulated pile be touched at the same time, the excess of electricity is discharged through the animal organs into the general reservoir, and produces the shock. Here the necessity of touching the top and bottom at the same time is admitted, but the cause of this necessity is not accounted for. According to the conditions of the hypothesis, it should follow, that touching the top of the pile only should occasion the full force of the shock; for no quantity of electricity can leave the top without being instantly supplied from the ground to the bottom, by means of the conducting wire: therefore if the hand be applied to the uppermost plate, the excess will pass through the body into the common reservoir, while an equal quantity is supplied by the wire to the lower end: and nothing more than this could happen were both hands applied in the usual manner. Now it is well known that by contact of the hand with one end only of

a pile, no shock is produced, provided the experimenter had been perfectly insulated. Thus it appears that although the conditions of the shock are admitted, the necessity of their accomplishment is not explained by the hypothesis.

With regard to the distribution of electricity in the pile, as supposed by the hypothesis, I have made some experiments which led me to think that we are, as yet, far from understanding the subject. In the insulated pile, as we have seen, the extreme plates are in oppositely electrical states, equal in intensity and quantity of difference: these states gradually become more feeble, as they approach the middle, and at this point, there are two plates in the natural state. Thus in a pile of 500 pairs, the 250th will be neutral. To a pile of 500 in number, I affixed four conductors, in such a manner that the first was connected with the copper or lower extremity, the second with the 100th pair, the third with the 400th, and the fourth with the 500th, which was the zinc or upper extremity of the pile. Thus there was a conductor at each end, and one at the 100th from the bottom as well as from the top. Now as the 250th pair was neutral, and as the extremities were at the maximum of difference, it is evident that each of the conductors at the hundredth pair from the top and from the bottom, must have been respectively positive and negative, and even in a high degree.

I procured an electrometer of a peculiar con-

struction, in which two unconnected gold leaves were suspended, each being capable of connexion with a different body, and of remaining perfectly insulated, as well from each other, as from the surrounding media; they were also capable of being removed from each other, or approached, still maintaining their insulation. One of these gold leaves was connected with the copper or lower end conductor, and the other with that of the next hundredth pair. These leaves being now, according to the hypothesis, in a similar state of electricity, though somewhat different in intensity, should repel each other; yet when they were slowly approached towards each other, they were mutually attracted, and at length stuck together, thus proving that the leaves were in a different state. The electrometer was then removed to the upper part of the pile; one gold leaf being connected with the conductor at the 400th pair, the other with the 500th or zinc extremity; the result was the same, the gold leaves were attracted to each other, and finally stuck together. To obviate any objections to these experiments relating to the possible alteration of state in the pile by the communication with the electrometer, it should be recollected that the gold leaves were insulated from each other, until the instant when their mutual attraction brought them into contact, and that the operator's hands never touched any part of the apparatus, unless where defended with dry glass. Hence no new state of the pile,

different from that supposed in the hypothesis, could have been induced.

When one end of the pile is connected with the ground, it is presumed that this end becomes neutral, that the negative electricity is destroyed throughout, and that the positive state increases with the number of the superimposed series. I shall here barely notice that in this state of the pile all decompositions, ignitions, shocks, &c. effected by it, are supposed to be produced by positive electricity only, and the auxiliary effects of negative electricity are precluded. To try if this supposed state of the uninsulated pile would agree with experiment, I affixed to the pile of 500 pairs, connected by its lower end to the ground, two conductors, one at the upper or zinc extremity, and the other at the 100th from the top, that is the 400th from the bottom. Here the electricities of both conductors were so highly positive that when Bennet's electrometer was applied to either, the gold leaves struck the sides. Yet when the insulated gold leaves of the other electrometer were connected to the conductors, in the manner already described, they strongly attracted each other.

It has, it is true, been proved by the experiments of Volta and De Luc, that at the centre of the insulated pile, a state exists which produces neither attraction nor repulsion, and which is therefore neutral. This fact, when compared with those already detailed, only shows that at

present we do not understand the action of the pile in producing electricity.

In the application of this hypothesis, Volta considers that it is the restoration of the electric equilibrium which produces the shock, the convulsive motions in animals, and the other effects. Whether this be so or not rests entirely on the grand speculative question concerning the identity of electricity and galvanism. As this is a discussion of the highest importance in Galvanism, and as it is the link of connexion between all the hypotheses, it claims a distinct consideration, and shall therefore in this place be omitted.

The increase of effect when saline or acid solutions are employed to separate the plates in the pile, is attributed by Volta to the greater conducting power of these fluids. The consideration of this question shall for the same reasons be deferred; for the present the following may suffice. That the interposed fluid should act merely as a conductor possessed of little or no electro-motive power does not seem to be supported, but rather opposed, by experiment. A pile of ten pairs of plates $2\frac{1}{2}$ by 2 inches surface, with cloths moistened in dilute sulphuric acid was erected, but not in the usual manner. Between the moist cloth and the incumbent metal, there was in each series a plate of closely grained varnished cork, and through this passed a bit of slender zinc wire, so that one end of the wire

was in contact with the wet cloth, and the other with the zinc plate placed over it. The arrangement therefore, beginning from the bottom, was in this order, zinc, copper, moist cloth, cork; zinc, copper, moist cloth, cork, &c. and the only difference between this and a common pile was, that the wet cloth was not in contact with the copper and the zinc, except by the mediation of the fine zinc wire. If the moist cloth were merely a conductor, it should follow, that the zinc wire would conduct the electricity from it to the incumbent zinc plate; and the same happening throughout the series, the pile should be as active as any other of the same number. The fact proved otherwise, for this pile had no power whatever, although three pairs in direct contact effected the decomposition of water. When a thicker wire was substituted in the corks, a feeble decomposition ensued, owing evidently to the influence of the large surface exposed by the section of the wire.

To this experiment the followers of Volta might object that the direct contact of the surface is necessary. I conceive this to be of no avail, for the conditions of the hypothesis, so far from making this necessary, indicate that the contrary of my results ought to happen. Further, it is not enough to assert that direct contact is necessary, without showing that this necessity is involved in the hypothesis: it should be shown that it has some more intimate connexion than merely to bear out the hypothesis in a difficulty.

The oxydation of the metals is considered by Volta as a secondary effect: and the increase of power produced in that operation, is supposed to depend on the establishment of a more intimate communication between the elements of the pile. To refute this supposition, it is only necessary to observe that, were it true, the effects should be at the maximum when the oxydation is greatest: yet it is well known that when the oxydation is complete, all effects in the pile cease. As to the more intimate union supposed to be established, it can scarcely be admitted, when it is considered that the oxide formed is a non-conductor, and that instead of forming a communication between the pairs of plates, it ought rather to insulate them. Of this we shall see more hereafter.

CHAPTER II.

Statement and examination of the hypothesis of Fabroni.

THIS hypothesis is directly opposed to that of Volta. In the latter, electricity is supposed to be the sole agent, and the chemical phenomena as no more than secondary; but Fabroni attributes all the effects to chemical action, and considers the electrical appearances as secondary or attendant. This hypothesis is in itself extremely simple;

it consists but of two general propositions, not applied to the explanation of particular phenomena: and as will afterwards appear, it seems, at least in part, a happy approximation to the truth.

Fabroni observes that since metals combine with each other, we may suppose that the tendency to combination begins when the particles are brought into contact, aggregation being the only obstacle. In the same manner, he considers that aggregation may also be the cause which prevents the oxydation of oxydable metals. But two metals during contact, their cohesion being weakened by their tendency to combination, may acquire the power of attracting oxygen from air or water, although neither could separately effect it. Thus running mercury retains its splendour for a length of time, but, when amalgamated, quickly oxydates. Lead and tin, when pure, remain long unaltered, but when alloyed, soon change. The copper sheathing of ships is soon destroyed, where in contact with iron nails.

It is this diminution of cohesion in metals, during contact, which causes them to yield to the weakest solvents. When plates of different metals as gold, silver, copper, tin, lead, are placed in separate portions of water, and kept separate from each other, no effect is produced. But if different metals are allowed to come into contact in the water, the most oxydable are quickly oxydated. Thus in all these cases there is an evident chemical action, and Fabroni concludes that the new "stimulating principle" is either caloric or oxy-

gen, both of which are evolved during the chemical action.

It would be to no purpose minutely to examine this hypothesis. The small stock of facts that were known at that time were most ingeniously applied by Fabroni to the support of his opinions. But a fatal objection arises out of the galvanic effects that may be produced by the contact of non-metallic bodies, with each other, and with metals. Thus, as far as we are at present acquainted with, charcoal and zinc have no attraction to each other; yet in a proper arrangement, they form one of the most powerful kinds of batteries.

That the tendency of metals to combine is a force opposed to cohesion, and that a diminution of cohesion takes place when contact is effected, *may* be a truth. It may also be true that aggregation is the power which opposes the oxydation of oxydable metals. But that this diminution of cohesion is the cause of the more rapid oxydation of a metal, when in contact with another, cannot be admitted on account of the following fact: If a wire of zinc, and a wire of copper be soldered end to end, and thrown into a fluid menstruum, the zinc is rapidly oxydated throughout its whole length. Now it is in contact with the copper at but one point, and it should be at this point only that the cohesion should be weakened.

His general conclusion, that the effects called galvanic are produced in consequence of chemi-

cal action, is no doubt true to its fullest extent. But that caloric or oxygen is the effective agent cannot be, and is not attempted to be supported by any one experiment. Fabroni has not even shown in what manner either caloric or oxygen could be conveyed instantly through conductors to a great distance, and produce a convulsive motion. In fine, it is not possible to apply the hypothesis, as he left it, to one phenomenon.

The consideration of the main proposition comes under the discussion of the question whether or not galvanic phenomena be caused by electricity.

CHAPTER III.

Statement and examination of the intermediate hypothesis adopted by the British philosophers.

DOCTORS Wollaston and Bostock, as has been stated, were the first who attempted to reconcile the two preceding hypotheses. Volta supposed electricity, produced by contact, to be the sole agent in galvanic phenomena, and the chemical effects to be no more than secondary. Fabroni considered chemical action to be the primary exciting cause, and looked upon the electrical appearances as secondary. Doctors Wollaston and Bostock, with Volta, considered electricity as the agent; but, with Fabroni, maintained that

this electricity is in all cases evolved in consequence of chemical action, or more specifically speaking, of the oxygenation of an oxydable body. The electricity thus liberated, Dr. Bostock supposed to combine with hydrogen, necessarily obtained from the fluid which gives oxygen to the metal; and this hydrogen, combined with electricity, passes on to the next conductor, where the electricity entering, leaves the hydrogen in an uncombined state.

The main postulate of Doctor Bostock's hypothesis is, that electricity has an affinity to hydrogen, and that hydrogen is essential to the action of the pile. Were it supposed that dilute sulphuric or muriatic acid were always the menstruum, by which electricity is separated from the zinc in the pile, this postulate might be granted. But when the menstruum is nitric acid, no hydrogen is evolved, yet galvanic phenomena are produced in an intense degree. Many other facts of the same kind might be adduced: but as the hypothesis was framed to account for the facts then known, it would be unnecessary to apply more recent discoveries to its subversion.

In the hypotheses of Volta and Fabroni, the power which is supposed in one to be the effect, is conceived in the other to be the cause. The modification proposed by Sir H. Davy is different from all the others; he endeavours to connect the two preceding by supposing that both the acting powers are causes. He, with Volta, admits that the contact of conductors in the series primarily

disturbs the equilibrium: but, with Fabroni, he allows that chemical changes are, to a certain extent, essential to the continued action of the pile.

As to the primary disturbance of the equilibrium, in a pile of zinc and copper with solution of muriate of soda, he states that by contact, the communicating metals are in opposite states of electricity; that to such low intensities water is an insulating substance; "every copper plate consequently produces by induction an increase of positive electricity upon the opposite zinc plate; and every zinc plate an increase of negative electricity on the opposite copper plate, and the intensity increases with the number, and the quantity with the extent of the series."

When the extremities of the apparatus are made to communicate, the opposite electricities tend to annihilate each other. The solution of muriate of soda, between the plates, is composed of substances which are in differently electrical states; the oxygen and acid are attracted to the zinc, and the hydrogen and alkali to the copper. A balance of power is obtained, which however is only momentary: for solution of zinc is formed, and the hydrogen is evolved. The negative energy of the copper, and the positive energy of the zinc, are again exerted, enfeebled only by the opposing energy of the soda in contact with the copper. The oxygen and acid, in like manner, enfeeble the power of the zinc, but this is of momentary duration, on account of the formation of muriate

of zinc. Such are the opinions of Sir H. Davy.*

The disturbance of the electric equilibrium, by the contact of conductors, is in this hypothesis liable to the same objections as in that of Volta. It has been already shown that this principle is not supported by one fact which cannot be better explained without it: and it has been attempted to be proved by experiment, that in the case of metallic electricity, as in any other, contact, instead of disturbing, restores the equilibrium. Sir H. Davy attempts to remove the difficulty by asserting that "as yet no sound objection has been urged" against this position:† but he has not shown in what manner he considers the objections of Mr. Wilson and others invalidated. This position, being the groundwork of the whole, seems to require some new support, before its dependent doctrine can be admitted.

The exaltation of power, supposed in this hypothesis to be produced in the metallic series by induction, is not founded upon any law of electricity with which I am acquainted. In all cases of induction, hitherto observed, there is not one, of what can be properly so called, in which the intensity induced is greater than the intensity of the inducing cause: this may be il-

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* Philosoph. Transactions, 1807. 43. and Elements of Chemical Philosophy.

† Elements of Chem, Phil. vol. i. 171.

lustrated by experiment. Let a series of Leyden phials be connected, so that the inside of one communicates with the outside of another throughout the whole. Sir H. Davy states that if a phial be charged by its knob, the outside becomes contrarily charged, by induction. If one of the abovementioned phials be now charged, the whole series will become charged also: but the third will not be more highly charged, because there are two others acting on it; nor the last more highly charged than the first. So much the contrary is fact, that in discharging each separately, the last will be found charged in the weakest degree.

Or, let several insulated conductors be arranged in a line, each being a short distance from the other, and let the last be connected with the ground. Let a pith ball electrometer be erected at each end of each conductor; and let the electrified prime conductor of a common machine be brought near that end of the series which is not connected with the ground. The range of conductors are now all electrified by induction, one end of each being positive, the other negative: the pith balls on the ends of each conductor diverge, and all equally; the last in the series being not more affected than the one which received its electrical state immediately from the prime conductor. These experiments are applicable to the plates in the galvanic pile or trough.

The term *induction* is of modern fabrication: beside numerous other objections that might be made to it, it is obscure, unmeaning, and does

not indicate any specific affection of matter or of the electric fluid. This mysterious word, when expressed in common language, means the necessary coincidence of the two states of electricity. This is the pure expression of the fact, divested of hypothesis : but it will be still more intelligible to denominate it, according to the Franklinian system, the necessary deficiency of electricity in any body, when there is an excess in its neighbourhood; for as electricity obtains an equilibrium, a portion cannot be removed to one place, without occasioning a deficiency elsewhere. This being understood, its application to the galvanic series is easy.

In a galvanic trough, each connected pair of zinc and copper exhibits the two states of electricity : the first copper is negative, the first zinc positive ; the second copper negative, and the second zinc positive. If each pair were insulated from the next, the copper of one pair would therefore be negative, and the opposite zinc of the other would be positive. Here then is the due coincidence of the two states, which obviates the necessity of induction, and even prevents its operation : the alternation of positive and negative is maintained between the opposite plates of the unconnected pairs, as well as between the plates that are in contact ; and it is between the plates which are in contact that the real distribution of the natural quantity into *plus* and *minus* should take place, if at all ; for the pairs have no connection nor relation whatever amongst each other.

Thus the result of arranging the metals in contact into distinct pairs should be the mere alternation of that feeble positive and negative electricity occasioned by contact.

Further, if it were to be supposed, with Sir H. Davy, that "every copper plate produces by induction an increase of positive electricity on the next zinc plate; and every zinc plate an increase of negative electricity on the opposite copper," the result must be that, as these changes would take place equally amongst all the plates, the electricity must be equally distributed, and of the same intensity, throughout the whole series, which is known not to be the case. But these changes would be still more complicated. Thus in a series of two pairs of plates, the copper being negative, acts upon the opposite unconnected zinc, which being already positive, is thus made more so: the zinc reacts upon the copper, and makes it more highly negative: this, as at first, ought to act upon the zinc, and make it still more positive, and so on reciprocally until the electricities would become very intense. The principle appears to me to be a revival of the long and justly exploded *antiperistasis* of the Peripatetics, and we may apply to it the observation of Boyle, that "according to the course of nature, one contrary ought to destroy, not corroborate the other,"* for the circumstances of

* Boyle's Works, vol. 2. 662.

this destruction are present and ought to operate, as will be shown hereafter.

Another objection to this hypothesis is, that it excludes the existence of the neutral or natural state of electricity, at the middle of the galvanic series. Yet in the hypothesis of Volta, it is stated that, in the middle of the pile, there are two plates in the natural state, and the statement is supported by the experiments of Volta and De Luc.

It has been also proved, by the experiments of Volta and De Luc, that when the lower end of the pile is connected with the common reservoir, the negative state is destroyed throughout, and the positive state increases in intensity towards the upper end. In a pile of 500 pairs, the lower end communicating with the ground, I found the 400th pair so strongly positive as, in some cases, to cause the gold leaves to strike the sides of the electrometer, and the positive electricity increased in each plate towards the 500th. Hence there can be no doubt that in a pile, thus circumstanced, the upper plates must be all positive, and that the agency of negative electricity must be excluded. I erected a pile in which the cloths were moistened with dilute nitric acid: from the top plate proceeded a wire which dipped into a glass of water: from the fifth from the top proceeded another, which also dipped into the same glass: and to the bottom of the pile were attached numerous connexions with the ground. The water in the glass was decomposed.

In this experiment it is plain that both wires were in the positive state of electricity: how then are we to reconcile the decomposition of the water with the opinion of Sir H. Davy, that the elements of compounds are separated by the difference of electrical state in the plates of the pile.

Another fact of the same tendency is the following. I erected a pile like the preceding, but the second wire was at the tenth plate from the top, instead of the fifth. The two wires at their other ends were plunged, at about an inch distance from each other, in solution of muriate of soda rendered blue by a vegetable colour. The wires being of platina, one gave off oxygen, the other hydrogen. At the oxygen wire, the fluid immediately adjoining seemed to lose its colour, as indeed might be expected; but round this colourless portion, it was strongly reddened: at the negative wire the solution was rendered green. Thus in the present case, as well as in the former, although the agency of negative electricity was excluded, yet the decomposition of the salt and solvent was readily effected. The salt was muriate of soda, the same which in the hypothesis is conceived to be decomposed by the attraction of the opposite electricities of the plates acting on its elements. So that, in explaining these decompositions, we may apply an observation of Sir H. Davy, that "it is not philosophical to assume a cause to account for an effect, when no such cause can be perceived."

There are considerations which would even lead us to conclude that neither positive nor negative electricity acts any part in the separation of these combined elements. This will be more amply discussed in the sequel; but I shall here anticipate as much as relates to the hypothesis under examination.

If a *couronne des tasses* be arranged, in which the glasses are filled with solution of muriate of soda, the state in which it can affect the electrometer is only when the extreme glasses are unconnected; and in this unconnected state the electrical intensity is at its maximum. There are two series of bodies present in the glasses, oxygen and acid naturally in the negative state of electricity, and hydrogen and alkali naturally positive. If then the differently electrical states of the metals be the cause which separates the oxygen and acid from the hydrogen and alkali, this separation ought certainly to be effected when the differently electrical states of the metals had the greatest power to do so. The metals, as we have seen, possessed the greatest power of doing so, when the extreme glasses were unconnected, therefore it is at this period that we should find the decomposition taking place: yet the contrary is fact, for in this state of things no such decomposition is effected.

When the extreme glasses are made to communicate, electric effects are no longer sensible: we are therefore obliged, by every consideration, to admit that the equilibrium is restored. Yet

at this period, and at this alone, the separation of the elements takes place. From these facts, I think it allowable to conclude, that when the electrical effects were at the maximum, the chemical effects were not perceptible; and that when the electrical effects were not perceptible, the chemical effects were at the maximum: and hence that neither positive nor negative electricity acts any part in the separation of the elements.

The grand question upon which Davy's hypothesis rests is, whether or not water be an insulator. If its insulating power be denied, every principle of this doctrine is subverted; if it be admitted, it opposes some of the facts which it was intended to explain: so that in either case a difficulty arises, and whatever may be the result of the investigation of this question, it cannot prove favourable to the hypothesis. This subject is therefore deserving of examination.

The only evidence in favour of supposing water to be an insulator, with which I am acquainted, arises from some experiments of Cavendish. This philosopher found that electricity, condensed upon glass, as in the Leyden phial, was much worse conducted by water than by metal: a cubic foot of water was required to conduct the charge of a surface equal to 1 foot. But from this, Mr. Cavendish did not conclude that water is an insulator; on the contrary, his experiment proved that it is a conductor, although in a much less degree than metal. Although water cannot

be ranked amongst the best conductors of condensed electricity, yet the experiments of Doctor Watson and others, in which electrical discharges were transmitted through amazing extents of this fluid, prove that it is a very good one. It is known that a Leyden phial, or a prime conductor, will not detain any portion of electricity, if the insulation be ever so little impaired by a slight coating of moisture. Even when the air is damp, the cylinder cannot be excited. But the hypothesis supposes that "with regard to electricities of such *low intensity*, water is an insulating body." Here in my mind is the mistake: it would rather appear that they are the highest intensities which are not conducted perfectly by water. The charge of a large battery, which during its discharge would pour out an immense quantity of electricity through the intervening stratum of air, is considerably diminished, if passed through water, the greater portion being quietly conducted: a stream of sparks from a conductor will pass invisibly, and with ease; and still lower intensities are not in the least obstructed, as far as experiment reaches. The following facts are still more directly applicable.

De Luc connected the wires, proceeding from the ends of a pile, by a portion of water; the divergence of the electrometers was completely destroyed. Can any proof more satisfactory be

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* Phil. Trans. 1809. p. 45.

required? The following also seems to deserve consideration. I sifted zinc filings, through a copper sieve, on a large metallic plate placed on an electrometer: through a zinc sieve, some copper filings were sifted on another electrometer: both diverged, one positively, the other negatively. The electrometers were then connected by a tube of water several inches long, into the ends of which were inserted wires. The consequence was, that when the filings were in due proportion, the divergence in both electrometers ceased entirely. This experiment seems to prove that water is a conductor to the kind of electricity evolved by simple metallic contact.

A proof of a similar kind may be collected from a well known experiment of Volta. A frog, without any preparation or dissection, was placed between two glasses of water, so that the legs hung in the water of one, and the body in that of the other. When a compound arc of zinc and silver was made to connect the two portions of water, but without touching the animal, contractions followed. This certainly could not have happened, unless the water conducted the feeble electricity supposed to be produced by the contact of the two wires.

Considering all these facts, I think it is not possible to admit that water is an insulator, nor even an imperfect conductor to low intensities. It is proved that water, in Volta's experiment, conducted the feeblest of all electricity; how then can the plates in the series act on each other

by induction; especially when the conducting power of the water is increased by salts or acids, for we know that in order to the production of this effect, the intervention of non-conductors is required. On the other hand, if we admit that water is an insulator, the admission is as fatal as the denial, as will appear from the following.

In the statement of this hypothesis* it is observed that if the fluid interposed between the plates of the series were incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity would cease. Accordingly it was found that a pile of 20 pairs of copper and zinc exhibited no permanent electro-motive power, when the interposed fluid was water free from air; for this does not readily undergo chemical change: the equilibrium was permanently restored through it, and when the ends were connected, the usual effects were not produced. In this restoration of the equilibrium lies the difficulty. It should be remembered that the pairs of plates have no other communication with each other, in the *couronne des tasses*, than by means of water; and that to such low intensities water is maintained to be "an insulating body." Now it is impossible to conceive how the equilibrium could be so completely and instantly restored, through a very extensive range of insulators. If water be an insu-

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* Phil. Trans. 1807.

lator, the equilibrium cannot be restored through it: if it be not an insulator, the principle of induction cannot operate; and there could never be a continued electric tension in the galvanic series: so that either case is fatal to the hypothesis, and the two statements, when compared, amount to nothing less than a contradiction.

If, in the water interposed between the plates, muriate of soda be dissolved, the different electricities of the metals are supposed to attract the elements of the solution, according to their electrical states; the oxygen and acid passing to the zinc, the hydrogen and alkali to the copper. The imperfection of the hypothesis plainly appears here also. It is admitted that if the interposed fluid be pure water, the electric equilibrium is restored when the extremities are connected. When salt is added, the restoration should also take place; for the same water which permitted this change before, is still present, and it has suffered no other change than that it can now permit the change with infinitely greater ease, its conducting power being very much increased. Hence there is no cause assigned for the separation of the elements of the salt, since the difference of electrical state in the plates ought to be annihilated throughout.

Another principal defect in this hypothesis is, its insufficiency to account for the decomposing power of the platina conducting wires, which proceed from the extremities of a galvanic series.

Here no acting energy is assigned, for the equilibrium is restored in the interior of the apparatus, by the chemical action going forward. The decomposition of water will suffice for an example. When the platina conducting wires are immersed in water, the positive zinc plates of the series attract negative acid and oxygen from the solution of muriate of soda used as an exciting fluid ; these opposite states of electricity destroy each other, a balance of power is thus produced, which however is only momentary. But the attraction being again instantly exerted, a new balance of the electric states must take place, which being incessantly renewed, and an analogous process continually taking place at the copper plates, there ought to be no energy exerted at the conducting wires in the water.

Before I take leave of this part of the subject, I cannot forbear noticing the strange inconsistency of hypotheses. Volta places water in his second class of conductors ; he explains the phenomena of the pile by its conducting power ; he allows that the more its conducting power is increased, as by the addition of salts or acids, the better it acts ; and he asserts that this increase of conducting power is the only mode of increasing the effects. Sir. H. Davy on the other hand assumes that water is an *insulator* ; and that, on this quality, the power of the pile depends. Here then are two diametrically opposite asser-

tions; and they are both maintained as true, by their different partisans.

The experiments of De Luc have proved, in the most satisfactory manner, that water, so far from being an insulator, is a very perfect conductor: for it restored the equilibrium between the ends of his electric column, so as completely to destroy the divergence of the electroscopes.

How degraded must be the state of that science in which fact is opposed to fact, assertion to assertion: in which positions are *assumed* to be true, that have been *demonstrated* to be unfounded: in which errors are allowed to pass currently, and are received without examination, on the credit of authority.

CHAPTER IV.

Statement and examination of Sir H. Davy's hypothesis of electro-chemical affinity.

THIS hypothesis has on a former occasion occupied my attention, and I have already offered such objections to it as, at that time, occurred.* Sir. H. Davy, in his Elements of Chemical Philosophy, considers that the experiments, on which

* Phil. Mag. vol. 37.

I grounded my objections, are inconclusive, and liable to fallacy: he conceives that my results may be explained otherwise than I have done; and therefore that they do not interfere with his opinions. He states also that I have much misrepresented his views; and declares that nothing is further from his hypothesis than supposing "that chemical changes were occasioned by electrical changes:" "they are conceived," he observes, "on the contrary to be distinct phenomena; but produced by the same power, acting in one case on masses, in the other case on particles."*

To the charge of misrepresentation, I have only to reply, that if I misrepresented his views, I misunderstood them: and if I misunderstood them, the case is singularly unfortunate, since the same conception of them is taken by Berzelius, Murray, Dr. Maycock, and by all others who have stated them. Indeed the new statement that chemical and electrical changes "are distinct phenomena, but produced by the same power, acting in one case on masses, in the other case on particles," is one that it would be excusable not fully to comprehend the force of; and after some reflection I cannot perceive a difference in the distinction specified. Particles and masses are, in effect, the same; they differ only in dimensions: a particle is such only when considered with

* Elements of Chemical Philosophy, part I. vol. 1. 165.

respect to a mass; and a mass becomes a particle when compared to a world. A particle is such when contemplated by our organs of sensation; but it is a mass to the microscopic eye of an animalcule. Is it then allowable to suppose that a distinction is to be observed between the electric affections of small and large masses: and would it not be equally allowable to distinguish the affections of large masses and of those much larger. If Sir H. Davy has made experiments or devised arguments which tend to establish a difference between the electricity of masses and of their particles, he has not stated them; he does not even declare that he is in possession of such. It is therefore excusable not to understand, and admissible to doubt an opinion which is neither fully stated nor supported.

To avoid misrepresentation, I shall in the present examination of the hypothesis, give an abstracted account of it, using as much as possible, its author's own words. In the sequel I shall reply to the objections made to my former experiments.

Bennet, and afterwards more distinctly Volta, showed that bodies, separated from contact, exhibited opposite states of electricity. Sir. H. Davy found that when acid and alkaline solutions were used as elements of piles constructed of one metal only, the alkali received electricity, and the acid gave it to the metal. Thus, when the order was tin, water, solution of potash, electricity passed from the water to the tin, and from

the tin to the alkali: but when the order was weak nitric acid, water, tin, the electricity passed from the acid to the tin, and thence to the water. These principles bear an immediate relation to the phenomena of electrical decomposition and transference. "In the simplest case of electrical action, the alkali which receives electricity from the metal would necessarily on being separated from it appear positive; whilst the acid under similar circumstances would be negative; and these bodies having respectively with regard to the metals that which may be called a positive and a negative electrical energy, in their repellent and attractive functions seem to be governed by laws the same as the common laws of electrical attraction and repulsion. The body possessing the positive energy being repelled by positively electrified surfaces, and attracted by negatively electrified surfaces; and the body possessing the negative energy following the contrary order."

Many experiments were found to confirm this analogy. In a series of charcoal, water, nitric acid; or charcoal, water, solution of soda, the positive energy appeared on the side next the alkali, the negative on that next the acid. In a series of zinc, moistened pasteboard, and moistened quicklime, the latter acted as alkali.

Solid dry acids and alkalies, by contact with metals, manifested electricity by the aid of the common condenser. Oxalic, succinic, benzoic, or boracic acids, if dry and presenting a large surface, became negative when touched by an in-

sulated plate of copper; the copper being positive. Contact of solid phosphoric acid with zinc produced similar effects. A surface of metal, separated from contact with dry lime, strontites, or magnesia, became negative. Potash gave no charge to the metal: soda succeeded in one trial and acted like an earth.

In the Voltaic decomposition of sulphuric acid, the sulphur separates on the negative side. Experiments have proved that the friction of sulphur with metals renders the former positive, the latter negative: the same result Sir H. Davy obtained by the contact of sulphur with an insulated metal plate. Wilcke states an exception with regard to lead, as being negative to sulphur; Sir H. Davy found lead to act like other metals, but conceived that Wilcke was misled by using tarnished lead; for this, or litharge rubbed against sulphur, was found to render it negative.

In the Voltaic decomposition of phosphoric acid, the phosphorus combines with the negative wire: from every analogy, it may therefore be inferred, that the electric energy of this inflammable, with regard to metals, is the same as that of sulphur.

On the general principle, oxygen and hydrogen ought to possess, with regard to metals, a positive and negative energy respectively: that they do may be shown by the agency of their compounds. Solution of sulphuretted hydrogen in water acts in the pile of one metal in the same manner as alkalies; and solution of oxymuriatic

acid is more powerful, in similar arrangements, than muriatic acid of greater concentration: in both cases, the oxygen and hydrogen must be active. Solutions of alkaline hydroguretted sulphurets exhibit a positive energy, with metals, greater than alkaline solutions: this is the case more particularly with copper, silver, and lead. In a series of copper, iron, and hydroguretted sulphuret of potash, the positive energy of the latter with regard to copper was sufficient to overpower that of the iron, so that the electricity passed from the copper to the hydroguretted sulphuret, and thence to the iron, contrary to the direction which it otherwise would have taken.

Bodies oppositely electrical, with regard to one and the same body, may be presumed to be so with regard to each other. Accordingly, a large surface of lime became positive by repeated contacts with crystals of oxalic acid, and the acid negative.

It will not be a remote analogy to consider other acids, and alkalies, oxygen, and hydrogen as possessing similar electric relations. In electrical decompositions, the bodies naturally possessed of affinity appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order. Thus acids, in the positive part of the circuit, separate from alkalies, oxygen from hydrogen; and metals, on the negative side, do not unite to oxygen, nor do acids remain in union with oxides: and in this way the attractions and

repulsions are propagated throughout the whole of the menstruum.

"As the chemical action between two bodies seems to be destroyed by giving one of them an electric state different from that which it naturally possesses, that is by bringing it artificially into a state similar to the other, so it may be increased by exalting its natural energy." Thus while zinc, one of the most oxydable metals, cannot combine with oxygen when even feebly negative; silver, one of the least oxydable, combines with oxygen easily, when positively electrified.

Bodies that combine chemically, exhibit opposite states of electricity; thus copper and zinc, gold and mercury, sulphur and metals, acids and alkaline substances, afford instances: and, if they had freedom of motion in their particles, they ought to attract each other by their electric powers. The relation of electricity to affinity is thus evident, "may it not be identical with it, and an essential property of matter?"

Beccaria's plates, oppositely charged, cohere strongly, and retain their charges after separation; this is a direct analogy: "different particles of matter in combining must still be supposed to preserve their peculiar states of energy." The general application of this doctrine may be easily made.

Supposing two bodies in differently electric states, exalted so as to create a force superior to that of cohesion, a combination would take place, more or less intense as the energies were more or

less perfectly balanced. This would be the simplest case of chemical union. But different bodies have different degrees of energy in relation to a third body, as different acids and alkalies have to the same metal; "such bodies may be in the same state or repellent with regard to each other, or they may be in opposite or attractive states which last seems to be the condition of sulphur and alkalies that have the same kind of energy with regard to metals."

When two bodies, repellent of each other, act on another body with different degrees of the same electric energy, the combination would be determined by the degree, and the feeblest energy would be repelled: hence elective affinity and decompositions. Or when bodies, having different degrees of the same energy with regard to a third, have also different energies with regard to each other, there might be such a balance of power as would produce a triple combination: and so in other cases of union. An explanation is afforded of the influence of the masses, as shown by Berthollet: for many feeble energies may exceed the force of a few that are stronger.

When bodies, differently electrified artificially, are made to restore the equilibrium, heat and light result: so also in intense chemical action. Heat operates in producing combination by giving freedom of motion; it also exalts the electric states, as in the tourmalin and sulphur.

In illustration of this supposition, Sir H. Davy states that he heated insulated plates of copper

and of sulphur; the electricity at 56° scarcely sensible to the condenser, at 100° produced divergence without condensation: the states became more intense as they approached the fusing point of these bodies, at a little above which they combine with the emission of heat and light. Similar effects may occur in the case of oxygen and hydrogen, and perhaps in all cases of combustion. Such are the views of Sir H. Davy.

1. In investigating this hypothesis, the first question that occurs is concerning the manner in which the electricity that is supposed to be the cause of chemical combination resides in bodies, —a subject upon which the author has not declared his opinions. To suppose that bodies, previously to their combination, possess a permanent state, whether positive or negative, is incompatible with the known nature of this fluid. Bodies may always contain electricity, but they do not manifest its peculiar properties unless after certain processes, and, even then, they return to their former inert state, after a certain period. To suppose that the electric states of bodies are owing to contact, as in the case of Volta's plates, is liable to all the objections already made in discussing that philosopher's hypothesis of galvanism: every consideration opposes such an assumption, and I know of none that countenances it. The remaining supposition is that the electricity of bodies may be called into action by their

tendency to separate from contact: but this is as objectionable as the rest, for in that case, the electricity could not act until the bodies had actually separated; and hence no cause would be assigned for their combination in the first instance, and their continuance in that state. Thus whichever of these opinions is held by Sir H. Davy, it appears that they are both encumbered with difficulties which he has not removed.

2. Whatever may be the truth of this question, it is certain that the electrical states presented by bodies are not invariably the same: to elucidate this part of the subject, a few examples shall be adduced. When smooth glass is rubbed with the hand, with metal, or with woollen cloth, it becomes positive: if rubbed with catskin, it becomes negative. Sealing-wax, rubbed against glass, becomes negative; but, rubbed with sulphur, is positive. Wood, by attrition with smooth glass, becomes negative; with rough glass, positive. Bergman found that two skeins of silk, rubbed across each other, exhibited opposite states. Æpinus produced opposite states on two pieces of the same plate glass, merely by pressing them together. And I found that by rubbing two plates of sulphur against each other, they became strongly and oppositely electrical. I also found that the electricity of sulphur, when rubbed by metal, is different from what it displays when rubbed by boracic acid. This variability of state is also shown in the same metal when brought in

contact with another. Mr. Wilson found that tin separated from contact with lead is negative, but with copper is positive: copper is negative with zinc, and positive with steel: zinc is positive with silver, but negative with bismuth; and so with others. These facts, and many others that might be adduced, render it manifest that bodies do not possess a determinate electric state as a natural and invariable property, but that it changes according to a variety of circumstances. Now were electricity the cause of combination, it is natural to suppose that as the combining power of a body is invariable, so would its electric state, and the more especially, as the same body is invariably attracted to the same pole, during the Voltaic decomposition of its compounds.

3. The next subject of consideration is, admitting the particles of heterogeneous matter to possess differently electric states, in what manner can this difference maintain a permanent combination? On this question, Sir H. Davy observes that "different particles in combining must be still supposed to preserve their peculiar states of energy." I know not from what experiments this conclusion is drawn. As far as observation has yet reached, the union of positive and negative electricity, whether intense or feeble, produces a mutual destruction of power; and bodies which have been differently electrified, when they are brought into contact, lose all signs of electricity. In the case of Beccaria's plates, given

as an instance of combined particles, the destruction of power takes place after a certain time. Should it be imagined that the particles of combined bodies, by becoming differently electrical at each new effort towards separation, might in this manner remain permanently united; it need only be observed, that the same could not apply to combinations dissolved in water: for under such circumstances, the bodies, during the effort of separation, could not regain a difference of state. Thus it does not appear how so evanescent a power as electricity can maintain a permanent combination: and it seems that, under the circumstances of the case, the contact of the particles, instead of producing combination, should destroy their power of attraction, if ever they possessed such a power.

4. The facts adduced by Sir H. Davy, to prove that bodies which combine chemically, exhibit differently electric states, are as follow. He found that sulphur, rubbed or heated in contact with metal, and then separated, was positive; the metal negative: and when sulphuric acid was placed in the Voltaic circuit, its sulphur passed to the negative side. By analogy, it was probable that phosphorus separated from contact with metal would also, but for peculiar circumstances of chemical action, exhibit the positive state; phosphoric acid, when placed in the circuit, gives off its phosphorus at the negative side. A plate of metal brought in contact with a large surface of

oxalic, succinic, benzoic, boracic, or phosphoric acid, proved that the acids were negative. When the metal was applied to surfaces of lime, strontites, and magnesia, these earths evinced the positive state. Soda, touched by metal, became positive like the earths, and there was reason to think that potash was affected in a similar manner. Sir H. Davy observes that since the earths and alkalies were positive with regard to a metal, and the acids negative with regard to the same metal, we may fairly conclude that these acids and alkaline substances were negative and positive, with regard to each other. This he accordingly found to be the case with oxalic acid and lime, for these substances, by contact with each other, became negative and positive.

With regard to the positive state of electricity attributed by Sir H. Davy to sulphur separated from contact with metals, he is supported by the experiments of Wilcke and Æpinus. However hazardous such a conjecture may appear, I have been led, by a variety of experiments, to suppose that these three accurate observers have, on account of the imperfect state of electrical knowledge, been led into error. I will lay down a statement of facts as I found them, and let them stand in support of my conclusions.

In the mode by which I tried the electricity of sulphur, the intensity produced was so high as to render Bennet's electrometer an unfit means of ascertaining its state : the most convenient instrument is the following. From the under surface

of an horizontal metal plate, three inches diameter, are suspended two gilt pith balls by gold twist strings: the plate is insulated by being supported on a glass rod bent for convenience at right angles in the middle, and its lower end set in a heavy metal foot.

A smooth circular plate of sulphur was separated from the metal mould in which it was cast, and laid on the metal plate of the electrometer just described. The balls diverged an inch. When a glass tube, excited by an amalgamated leather, was brought over the plate, the balls collapsed: or if the tube was approached towards the balls, they were strongly attracted. If a piece of sulphur, excited by woollen cloth, was brought near the sulphur plate, the divergence was increased; or if applied to the balls they were repelled. I had therefore no hesitation, after repeating this experiment many times, in pronouncing that the sulphur, by separation from metal, had become negative. The mode of pouring fused sulphur upon metal, here employed, is a more perfect means of obtaining the same result as by heating plates of these substances together, as will appear.

To try if the upper surface of the sulphur plate were in the same electric state as the under one, I turned that side down on the metal plate of the electrometer; it presented the same phenomena.

These experiments were varied by pouring fused sulphur on other metals, as zinc and platina; the sulphur when separated was negative.

The effects of friction were next tried. A sulphur plate which did not manifest symptoms of electricity, even to the condenser, was rubbed with a polished plate of copper, and laid on the pith ball electrometer: the electricity proved by all the tests to be negative. New plates of sulphur, but devoid of electricity, were each rubbed with a different metal, as gold, silver, platina, zinc, lead: in all cases the electricity of the sulphur was negative. Wilcke stated that the friction of metals rendered sulphur positive in every case but that of lead, and that this rendered it negative. Sir H. Davy supposed that this was an error into which Wilcke was led by using tarnished lead; for, when the experiment was repeated with bright lead, the electricity of the sulphur was positive: but if tarnished lead or litharge were used, he found the sulphur negative. In my trials, this anomaly did not occur. On three different pieces of lead, one very much oxydated, the next only tarnished, and the third bright, fused sulphur was poured. When perfectly cold, the sulphur was detached from each: that from the oxydated lead was not electric, the lead having lost its coating of oxide; that from the tarnished lead was negative; and that from the bright specimen was strongly negative. The trials were repeated, using friction: three pieces of lead, like the former, produced negative electricity on new pieces of sulphur. A plate of sulphur was also rubbed against a surface of litharge, the sulphur became negative, but very feebly so.

To prove that the experiments, in which the sulphur plate was placed on the metal plate of the pith ball electrometer, indicated the real state of the sulphur, and that no deception could arise from what has been called *electric influence* acting between the glass tube, used for proving the kind of electricity, and the sulphur plate, I excited a plate of glass, with an amalgamated leather, and laid that surface on the electrometer. The electricity of this glass may be considered as a standard; it was positive: the balls diverged; and when the tube, also excited with amalgamated leather, was brought over the glass plate, the divergence was increased, but was diminished by excited sulphur. This shows that the balls received the true state of the superimposed electric; and that their testimony in the other cases may be equally admitted.

A large metal plate being placed on a Bennet's electrometer, sulphur was sifted, in one trial from a copper sieve, and in another from a zinc sieve: in both cases, the leaves diverged with negative electricity. To prove that the foregoing experiments, with fused sulphur, afforded the same results as simply heating in contact, a plate of sulphur and one of copper were heated together, the sulphur was separated, and placed on the cap; the gold leaves diverged negatively, but very feebly.

These experiments have led me to conclude that the real electrical state of the sulphur in all these cases was positive, and that its supposed

negative state originated in a circumstance which has been overlooked, and which it is now necessary to detail.

In an essay some time since published* I stated the following fact. If from a very small insulated conductor, a pair of gilt pith balls be suspended by gilt strings, and if an excited glass tube be brought in contact with the conductor, the balls diverge positively. After some time, the tube being held steadily in its place, the balls collapse: but if the tube be suddenly removed, they diverge again, but with negative electricity. I am not aware that this fact has been noticed by any one before me; and I conceive that it involves a principle of electricity, a want of knowledge of which has been the source of many errors in ascertaining the electricity of bodies.

If on the metal plate of the pith ball electrometer, a plate of sulphur, excited with metal be placed, the balls diverge negatively; but if the sulphur be suddenly removed, they diverge positively. There are states of the weather in which the experiment succeeds better than in others. If the air be dry and the sun unobscured, it succeeds perfectly; but the balls must be allowed to come together before the removal of the sulphur, which often requires a long time: at other times, half a minute or less will suffice. If Bennet's electrometer be employed, the sulphur should be

* Phil. Mag. 1814.

very feebly excited, and may be removed the instant after it is applied: the leaves then diverge positively.

It is very probable that, in examining the electric state of sulphur after contact or friction with metals, Sir H. Davy merely applied the sulphur to the electrometer plate, that he removed it when he supposed the electrometer to have received its state, and then examined the electricity of the leaves. To the same cause also are, no doubt, the statements of Wilcke and Æpinus to be attributed.

Sulphur being thus proved to possess a negative energy, it was necessary to examine the state of other bodies. Sir H. Davy has considered all inflammables as possessing a positive energy: from the analogy of sulphur, it became probable that they might be negative. A large polished plate of copper was placed on a gold leaf electrometer. A quantity of finely powdered charcoal, obtained from boxwood ignited during two hours, was briskly sifted through a well polished zinc sieve of six inches diameter, the holes being exceedingly close together, and therefore very numerous. When the charcoal powder arrived to the copper, the electricity was so strong that the gold leaves struck the sides repeatedly. The powder was now shaken on so gently that the gold leaves obtained a permanent divergence of about half an inch. The approach of a glass tube, excited by an amalgamated leather, caused the leaves to collapse; but sulphur excited by

woollen cloth increased their divergence. So far, these two inflammables agree in their electric energies. It appeared useless to make trials of phosphorus, as they could scarcely end in any thing but disappointment.

In the hypothesis under examination, the earths are stated to be bodies possessed of a positive electrical energy; hence their attraction to the negative side of the Voltaic series. On account of the results obtained from inflammables, I was led to suspect that the real state of the earths might be negative; and in investigating this question, the same means as those employed by Sir H. Davy were used, except the substitution of sifting, instead of contact of surfaces.

A polished plate of copper being placed on the gold leaf electrometer, a quantity of finely powdered dry caustic lime was sifted over it from a copper sieve. The gold leaves diverged half an inch, but excited glass reduced the divergence, and sulphur increased it. The same effect was produced by a zinc sieve: and every thing proved that the real state of the lime was negative.

The copper plate being cleared, replaced, and the apparatus deprived of its electricity, a quantity of dry well burnt magnesia was sifted through a copper, and afterwards through a zinc sieve. In both cases, the leaves diverged considerably with negative electricity.

I found it very difficult to produce signs of electricity with barytes, but succeeded once with a zinc sieve: the divergence did not exceed one

eighth of an inch, and this was entirely destroyed by the approach of excited glass. Perhaps had I used more barytes, the divergence would have been greater.

These experiments seemed analogically to warrant the conclusion that all inflammables and earths are naturally possessed of a negative electricity; and therefore that the states of bodies on which the electro-chemical hypothesis is founded, have not been accurately determined. As long as it was supposed that inflammables and earths are naturally in the positive state, their attraction to the negative pole was coincident with what should be expected, and the whole hypothesis was so far connected. But when it is found that these bodies are naturally possessed of a negative energy, it cannot be supposed, consistently with the established laws of electricity, that a similar state will attract them: in this case inflammables and earths should pass over to the positive side.

5. The difference of electrical state, manifested by dry acids and metals, aptly corresponds with the views of the hypothesis. The acid and metal might be supposed to combine in consequence of this difference; and when the combination would be submitted to Voltaic decomposition, the negative acid should pass to the positive pole, and the positive metal to the negative pole. Both these changes accordingly happen. But this mode of action is con-

tradicted by other, and far more numerous cases, in which the order of decomposition would be very different from what observation has established as a criterion. A few examples will set this in a clear point of view. Zinc and mercury, by contact, present difference of electric state; the one becomes positive, the other negative. These metals may therefore, as the hypothesis would suppose, combine on account of their opposite electricities. But when a small portion of amalgam of zinc and mercury, in a vessel of water, would be submitted to Voltaic decomposition, by immersing the polar wires in the water, the zinc, naturally positive, should pass to the negative side, and the mercury, naturally negative, should be found at the positive pole. The same thing should happen with every other amalgam. Sulphur and metal, by contact, become differently electric: the sulphuret, when subjected to decomposition, should transmit the sulphur to the positive side, and the metal to the negative. Yet we know that, in these cases, no such phenomena take place.

To object that such compounds as have been just instanced are insoluble, and some of them non-conductors, is, I conceive, of no avail: for bodies of this kind are as much subject to decomposition by galvanism as any other, provided due time be allowed. The labours of Sir H. Davy have furnished us with examples of this kind. He subjected to Voltaic decomposition glass, sulphate of lime, sulphate of barytes, Carrara mar-

ble, fine grained basalt, compact zeolite, lepidolite, vitreous lava of Ætna, all of which are insoluble, and, I believe, non-conductors; each yielded up its elements, and these arranged themselves according to the usual order.

There are other cases still more applicable, and not liable to the apparent objection of insolubility. By reiterated contacts of sulphur and caustic lime, each presenting a large smooth surface, I found, with the aid of the condenser, that the former became negative, the latter positive. Friction manifested the same thing very easily to the common electrometer. When the compound of sulphur and lime, which is soluble in water, is submitted to Voltaic decomposition, the earth should pass to the negative side, the sulphur to the positive. It is also possible, although difficult, to make sulphur and magnesia present different states: the experiment does not succeed without friction, the magnesia having been exposed to an intense heat. The sulphuret of magnesia should, in its decomposition, follow the arrangement of the sulphuret of lime. The same may be observed of various other substances.

6. The chief experiment which seems intended to illustrate the electro-chemical hypothesis, and on which it rests, is that in which plates of sulphur and copper, by being heated in contact, acquired differently electric states when separated; the intensity increasing with the temperature Notwithstanding this increase of intensity, it is

barely sufficient at 100° to affect Bennet's electrometer. At 220° the fusing point of sulphur, the electricity must have been still exceedingly feeble. Is it then possible to suppose that this feeble electricity could be a cause sufficient to effect the combination of these two bodies; and that the vast quantity of heat and light, extricated during the combination, is no more than the restoration of the equilibrium between electric states of such feeble intensity? Beside, I have reason to think that, at the fusing point of sulphur, the electricity of the plates in contact becomes weaker instead of stronger. If fused sulphur be poured very hot on polished copper, and suffered to remain some hours, the sulphur, when separated, will be sometimes found scarcely electrical: the copper is rendered purple. From this it would appear, that the higher the temperature, and the nearer the approach to combination, the weaker will be the electricity: and should it be supposed that by the cooling of the two bodies, electricity was lost, it is only necessary to instance the following fact. If sulphur, which has been fused and has arrived at the very fluid state that immediately precedes its solidification, be poured on polished copper, and separated after many hours, the electricity will be often stronger than if the sulphur had been detached as soon as it became solid: it will be so intense as to cause the divergence of large cork balls. Now as, in Sir H. Davy's experiment, the electricity

at 100° was barely sufficient to separate gold leaves, and as in mine, it was at least 100 times more intense at the common temperature, I think it is not unfair to conclude that the electricity is not proportioned to the heat, and therefore that there are no grounds for supposing the combination of the sulphur and metal to depend on the electricity produced.

A fact which renders this conclusion still more admissible is, that if fused sulphur be cooled upon zinc, the electricity is much stronger than if copper had been employed. Yet so feeble is the affinity of sulphur to zinc that their combination cannot be effected without much difficulty. Platina and sulphur afford also a strong electricity, yet these bodies, until the experiments of Mr. E. Davy, were considered incapable of combination, so feeble is their affinity for each other. These considerations appear to me greatly to invalidate the force of the conclusions, drawn by Sir H. Davy from his experiments on the plates of sulphur and metals.

7. That bodies which unite chemically should, by contact, display differently electric states, is no proof that their union is owing to this difference of state. There are perhaps no two bodies in nature that would not manifest the same, if excited by contact or friction: yet all these bodies do not unite. Two pieces of glass, two pieces of sulphur, or glass and sulphur, will cause the evolution of electricity. I found that sulphur, a

body naturally negative, will, when rubbed with dry boracic acid, became strongly positive on both surfaces, the acid being negative. If the sulphur, after this, be rubbed with the hand, all signs of electricity disappear: but if the rubbing be continued, a negative state is induced. Notwithstanding this, we know of no combination between boracic acid and sulphur. I also found that charcoal, separated from contact with zinc, became strongly negative, the metal therefore positive; yet between these two substances there is no known combination. Earths, with metals, produce opposite states; but, as far as we know, they do not combine chemically. The same may be observed of certain alkalies with metals; and various other substances.

On the whole, it is not allowable to conclude, because two bodies, possessed of a mutual affinity, display difference of electrical state, that this is the cause of their combination, when every thing bears testimony that a far greater number of bodies display much more intense electricities, yet are not known to combine.

Amongst some observations on this hypothesis, which I made a few years since,* I adduced other instances of bodies possessing different electricities in a high degree, which did not combine. It may be of use to notice a few of them here with a view of removing the objections to them

* Phil. Mag. vol. 37.

which have been, and might be made. When the polar wires of an acting Voltaic series are immersed in water, this fluid is decomposed; its oxygen passes to the positive side, its hydrogen to the negative. If the positive polar wire be silver, the oxygen is not evolved, but unites with it. The reason assigned for this union is, that the silver being rendered strongly positive, attracts and unites with the oxygen naturally negative, although under ordinary circumstances no such combination would take place. This explanation being admitted, it ought to happen that, at the negative side, the converse process would take place. Suppose the negative polar wire to be of zinc. By its situation, it is in a state of electricity as strongly negative as the silver was positive. Now, since the positive silver united with negative oxygen, why should not the equally negative zinc unite with the equally positive hydrogen, if difference of electrical state be the cause of chemical combination? Both wires are under the same favourable circumstances for the exercise of affinity; and if one exercise that power, and the other do not, it proves, I think, that the combination does not depend on that power which was equal, and might have acted in both cases. To suppose that cohesion offered any obstacle to the efforts of the negative electricity, were in vain; for we know that the cohesion of silver is much greater than that of zinc. Further, it were not a valid objection to urge the natural affinity of silver for oxygen, and the want

of affinity between zinc and hydrogen. For natural affinity, or what is the same, natural electricity, can not act, as the silver, by its communication with the zinc or positive end of the series, must be naturally negative, and therefore naturally repulsive and not attractive of oxygen which is also negative.

In the same manner, if for the silver wire at the positive side of the series, one of platina were substituted, the quantity and intensity of positive electricity, induced on the platina wire, is the same as that which the silver possessed. Now since this quantity and intensity was sufficient to cause silver to combine with oxygen, why should it not be adequate to effect the combination of platina with oxygen? In this case, no more than in the former, can cohesion operate; for we know that iron, which possesses much greater hardness and tenacity, would be violently acted on, under similar circumstances. These are difficulties of magnitude, as long as it is not admitted that some other power, beside electricity, is acting on the wires.

8. In the electro-chemical hypothesis, it is supposed that difference of electrical state is the cause of chemical combination; and contrarily, that similarity of state is a means of preventing combination. Upon this latter question, I made the following experiment. Two insulated cylindrical vessels were connected by a stop cock of which the handle was glass, in such a manner that,

when open, the fluids of one might run into the other. One vessel contained solution of oxalic acid, the other lime water. By means of conducting wires, which plunged into the fluid of each, a full stream of electricity was poured into both from an exceedingly powerful Nairne's machine. Pith balls were also connected with the fluids to prove their state. The stop cock was opened by its insulating handle; the machine was kept in continual action; and yet when the two fluids came in contact, a precipitation of oxalate of lime took place.

In this experiment, both fluids were in the positive state; the agency of negative electricity must therefore be excluded. If any minute quantity had even been produced by the contact of the two fluids, it must have been instantly destroyed by the continual streams pouring in from the machine. I made choice of oxalic acid and lime, as these bodies are instanced by Sir H. Davy as producing different states by contact.

To the evidence afforded by this experiment, Sir H. Davy, in his "Elements of chemical Philosophy," has stated some objections, which it is here necessary to consider. He first observes, that "a non-conducting acid, though brought in contact with a positive surface, electrified by the common machine, is not rendered positive throughout; but gains a polar electricity, which extends only to a certain depth in the crystals, and the exterior surface, if electrical at all, is negative." Facts with which I am acquainted in-

duce me to believe that it would be difficult to support this statement: but it is unnecessary to advert to this circumstance, as there is no analogy between a crystalline non-conducting acid, and the same substance dissolved in a conducting medium. He then continues, "if a wire positively electrified by the common machine, be introduced into an acid solution, this solution, if at all affected, when made to act upon another solution, will be negative at its point of action; that is, it will be positive near the wire, but will be in the opposite state with regard to another surface."* Thus from the supposition that the crystalline acid cannot be rendered positively electrical throughout, it is inferred that the same in solution will be similarly affected. I cannot see how the inference is warranted, being neither supported by experiment nor analogy. In my experiment, the acid solution was in direct contact with the conductor of the machine, and being itself a good conductor and strongly electrified, it is difficult to conceive how it could assume a polar electricity. And if the acid solution did assume a polar electricity, the lime water in the other vessel should do the same, and become negative in these portions farthest from the conductor. In this case, the parts of the acid and alkaline solutions, that come into contact, would be both negative, so that still

* Elements of chemical Philosophy, vol. I. part 1. 165.

my objection would apply. I have, however, made an experiment which seems to prove that there is no polar electricity concerned. A glass tube, two feet long, had a short wire cemented in at each end, being previously filled with solution of oxalic acid. The wire, at one end, was connected with the conductor of a powerful Nairne's machine: from the wire at the other end, were suspended pith balls by gilt threads, and the tube was supported in the middle. The machine being put in action, the balls diverged: they were repelled by glass excited with an amalgamated leather, and attracted by excited sulphur. According to the opinion of Sir H. Davy, the fluid in the tube should be positive at the end next the positive conductor, but negative at the opposite end: yet it proved to be positive at both. The same results were obtained when the tube was filled with lime water, or even with common water, which is not so good a conductor. Sir H. Davy, alluding to the experiment in which the acid and alkaline solutions were similarly electrified, observes, that "common electricity is too small in quantity, in its usual form of application, to influence chemical changes." But in the case above mentioned, if the electricity thrown in were too small in quantity, its failure in preventing the union of oxalic acid and lime could be only owing to its being absorbed or destroyed by the difference of state in either of the bodies concerned. That there was no such destruction is proved by the continual repulsion of

the pith balls. It is therefore obvious, that if so very low a state of differently electric intensity as that supposed, could cause combination, a much higher intensity, of a similar state in both bodies, should prevent it.

As to Sir H. Davy's last objection to my experiment with solutions of oxalic acid and lime, namely, that "it requires a very strong machine acting upon a very small surface, to produce any sensible polar decompositions of bodies," I conceive that it applies with much more force to his own hypothesis. For how is it possible he can at the same time suppose, that the very low intensity of a few four inch plates, diffused over so large a surface of metal, can effect the decomposition of the interposed menstruum.

I have made another experiment to which I think it cannot justly be objected that the electricity was in too small a quantity. A small vessel made of platina foil, capable of containing two or three drops of fluid, was cemented on a glass rod, nearly filled with sulphuric acid, and connected with the positive pole of a galvanic pile of 200 pairs, the negative pole being connected with the ground. The apparatus was propped by glass pillars in such a manner as to prevent the pressure on the lower part, which might otherwise weaken its action. A bit of pure soda was thrown into the platina vessel, and a combination was immediately effected with force. In this experiment, the acid was brought into a state si-

milar to that of the soda, the electricity was of that abundant kind which is supposed to effect chemical changes, and ready to be instantly supplied if destroyed. Indeed the circumstances for producing a large quantity of electricity, and even of high intensity, were present in an eminent degree. The connexion with the ground afforded the quantity, and the non-communication of the polar wires with each other produced the intensity: and the wire from the positive pole, when applied to a gold leaf electrometer, caused a considerable divergence.

From all the preceding considerations, I conceive that Sir H. Davy's objections have not invalidated the conclusions drawn from my first experiment on similarly electrified bodies, and that to the second experiment these objections do not apply.

9. It is stated as a corroborating circumstance in this hypothesis, that the most oxidable metals, when placed on the negative side of the galvanic series, will not unite to oxygen nor acids: and the cause assigned is, that the metal and acid, being naturally in a similar state of electricity, do not attract each other. This fact, which at first seems so happily to coincide with the proposed view, does not prove its object. I made some experiments on this subject, and selected copper and nitric acid, as bodies in similar states, which therefore ought not to unite.

A copper wire connected with the negative side of a galvanic series, was immersed in di-

lute nitric acid; a platina wire completing the communication with the positive end. After suffering the action to proceed for some time, the dilute acid was examined, and was found to contain no copper: so far coinciding with the hypothesis.

A galvanic series, in every respect the same, was arranged as before; but the copper and platina conducting wires were immersed in stronger nitric acid than in the preceding case. The effect of dilution on the acid must have been to lower its negative intensity; the effect of concentration must have been to heighten it: the repulsion between the copper wire and this more concentrated acid should now be the stronger; and therefore combination was less than ever to be expected. Yet experiment proved, that the copper now dissolved with facility. The same result was obtained with zinc, on a much larger scale. A zinc wire was connected to the negative end of a battery of 300 pairs of plates contained in porcelain troughs; the wire was immersed in very dilute nitric acid, but it did not dissolve: when the acid was more concentrated, it dissolved with the greatest facility. Thus the support of the non-union of metals to oxygen, on the negative side, seems to be removed: and it will appear in the sequel that resource need not be had to the electro-chemical hypothesis for an explanation of the fact, as far as it is true.

10. In my observations on this hypothesis already alluded to, I have noticed that the attraction of the elements of a compound, during its decomposition, to the polar wires of the galvanic series, is not accounted for. In all cases of electric attraction, hitherto observed, a non-conductor must be interposed between the acting electric and the body acted on. In common experiments, this non-conductor is air: and when the air was removed, Beccaria found that attraction and repulsion scarcely took place. When dissolved salts are submitted to galvanic decomposition, the water, being a conductor, should suffer the opposite electricity to act through it, without causing the elements to separate or move towards the polar wires. It hence appears, that, with regard to the electromotion of the elements of compounds, the hypothesis is inadequate.

But passing this objection, and supposing the elements to have arrived at the polar wires, they must assume the electric state of these wires, and to an equally high intensity. In this case, they should be each repelled from each wire towards the middle point, where meeting each other in this new state of high and opposite intensity, they should combine with greater force than ever. Thus, a permanent decomposition could never result; for there are two powers continually acting against the repose of the elements at the polar wires: they are repelled by these wires on account of their similarity of electric

state, and the elements attract each other. Or, if it be supposed that these repelled elements cross each other in the middle, without combining, it must happen that when they arrive at the other pole, they will, in like manner, receive the electric state of that side, will be therefore again repelled, and this continually taking place, there will be an incessant attraction and repulsion of both elements from each pole, and they can never be found separately at either.

It is not necessary, in this place, to make any observations on the explanation of the decomposition of water given by the hypothesis, as this subject shall be noticed in the sequel.*

I have made no observations on the electro-chemical hypothesis of Professor Berzelius; for, being but little different from that of Sir H. Davy, whatever objections apply to one, apply equally to the other.

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* I formerly detailed an experiment intended to prove that water is not recomposed in the middle point, as supposed by Sir H. Davy. A tube, containing a platina wire and muriate of lime, was made to connect the two portions of water; and pieces of charcoal were used to terminate the conducting wires of the galvanic series. In this case, the circulation of gases was between the charcoal and the platina wires, as appears from the facts ascertained by Mr. Murray relating to the action of interrupted conductors. (System of Chemistry, vol. 1. p. 605 note). It has been objected to my experiment that the muriate of lime was a non-conductor; but the objector forgot that a platina wire passed through the whole length of the tube, and was connected with the wires of the bell glasses.

CHAPTER V.

On the question, whether the agent in galvanic phenomena be electricity.

THE opinion that electricity is the agent in galvanic phenomena is of as early a date as the first facts discovered. Indeed such a conclusion seems to have been warranted by the striking coincidence between the effects of electricity and of metallic contact. It was first discovered that metals in contact produced contractions in the muscles of animals: it was also found that minute portions of electricity did the same: and shortly after it was ascertained that the contact of metals caused the evolution of sensible electricity, the intensity being greater in proportion to the power of the metals in producing contractions. But the grand discovery of an arrangement of associated metals, and of the consequent accumulation of a power which, while it produced, in a high degree, all the effects called galvanic, appeared also to possess all the properties of electricity, set the matter in so convincing a point of view, that it was difficult to refuse assent to the proposition that electricity is the agent in galvanic phenomena. The power called into action by the pile was found to exist in two states possessing properties identical with positive and negative electricity. A Leyden phial could be charged by

the pile in the same manner as by an electric machine: and the charge thus communicated gave a shock, effected attractions and repulsions, and presented the same luminous appearances as electricity obtained in the ordinary manner.

As to the hypotheses which have been devised to explain the phenomena of galvanism according to electrical principles, we have, in the preceding discussions, seen reason to think that their principles are neither consistent among themselves, nor accordant with experiments. The fundamental principle of these hypotheses therefore remains to be examined; and I shall state those considerations which have induced me to suppose that electricity is not the cause of galvanic phenomena; at the same time admitting that electricity is really present in these cases, but conceiving that it acts a subaltern part, and that it is rather the effect than the cause.

When a plate of zinc is applied to a plate of copper, the electric equilibrium is disturbed, the former becoming *plus*, the latter *minus*. If the leg of a frog be made a part of the lateral circuit between these metals, it is supposed that the excess of electricity passes from the zinc, through the animal conductor, into the copper; and that the passage of this electricity is the cause of the muscular contractions produced.

I provided myself with a pair of plates—zinc and copper soldered face to face. This compound plate was laid on a glass plane, with its copper surface downward, and from this surface

proceeded a short copper wire which, at its other end, terminated in a plate of insulated copper. When the limbs of a prepared frog were placed on the latter copper plate, they may be considered as merely in contact with the copper surface of the compound plate, but in a manner more convenient for experiment. A zinc wire, bent into the form of a discharging rod, was used to connect the zinc surface of the compound plate with the sciatic nerves of the frog. Every time the connexion was established, contractions, as might be expected, followed.

If the cause of these contractions were the restoration of the electric equilibrium through the frog, the electricity should observe the ordinary law, and it should follow, if a shorter circuit were established between the surfaces of the double plate, that the effect on the frog would cease. In making this experiment, it would not be sufficient to complete the circuit either by a zinc wire brought from the zinc surface to that of copper, nor by a copper wire from the copper side to the zinc; for in both cases one of the soldered plates would be between two metals of the same kind, which according to the hypothesis would produce a destruction of power. A copper wire was therefore soldered to the copper or under surface of the double plate, and bent round, so that its other end, turning over, came in contact with a small bit of cloth wet in dilute nitric acid, and placed on a corner of the zinc plate: all the remaining surface of the latter

being left dry. Here then, according to Volta's principles, was every circumstance necessary to the free circulation of electricity. The frog was again made a part of the lateral circuit between the surfaces of the compound plate, by means of a zinc and copper wire each 2 feet long; after each contact, violent contractions ensued.

The result of this experiment seemed irreconcileable to the laws of electricity. There was a good, and very short circuit established between the surfaces of the double plate, yet the electric fluid preferred a course 24 times longer, in which the limbs of the frog, a comparatively bad conductor, offered a considerable obstruction.

The result of this experiment should have some weight; it was withdrawn from the hypothesis of Volta, the balance of opinions was no longer maintained, and that of Fabroni therefore preponderated. It seemed more easy to conceive, and as I shall hereafter endeavour to show, more consonant with the catalogue facts, to suppose that the saline juices of the frog, which experiment proved to be present, might act on the zinc wire; for we know that the saline animal fluids are powerful exciters of zinc. It therefore occurred that if for the zinc wire, one of bright copper were substituted, the effects would be much less: for although muriate of soda acts readily on oxide of copper, and is on that account employed by copper smiths, yet it has very little action on the pure metal. A trial was therefore made with a

newly prepared frog; and as was expected, the contractions were much more feeble.

I then coated the sciatic nerve with a bit of tin foil, in the usual manner, and on applying the copper wire, the contractions were much stronger than in the last trial. Wires of various metals were used with equal effects. Hence, it was plain that the tin foil acted a principal part; and it became evident that, to ascertain the true result of the different metals, the naked nerve must be the subject of experiment.

It now occurred that if a conducting wire made of some metal not capable of being acted on by the animal fluids, were employed, there might be perhaps no effect. A plate of zinc was therefore connected with one of platina, the latter being connected with another of platina, on which lay the limbs of a newly prepared frog. A platina wire was used as a conductor from the zinc surface to the nerve; when the contact was completed, there was not the slightest convulsion produced.

When a wire of pure silver was tried as a conducting arc, there was no appearance of contraction. A small gold wire was not more efficacious.

From these trials, it appeared that while no chemical action was going forward in any part of the apparatus, there was no sensible effect. I wished therefore to examine what change would happen were chemical action permitted to take place. The copper and zinc apparatus was ar-

ranged as before, but with a drop of dilute sulphuric acid on the zinc surface. One end of the copper conducting wire being applied to the nerve of the frog, the other was touched to the surface of the acid; violent contractions ensued. When the platina and zinc apparatus was tried in the same manner, the platina conducting wire produced contractions not less violent. A silver or gold conducting wire, tried with acid, afforded the same results: and many saline solutions were equally efficacious with the acid.

When repeating my trials with the platina and zinc apparatus, without any drop of menstruum, I once obtained strong convulsions on connecting the nerve to the zinc plate by the platina wire. The cause, on examination, proved to be my having inadvertently applied to the zinc plate that end of the wire which, in a previous experiment, had been moistened with the saline juices of the frog, during contact with the nerve, and which therefore produced chemical action on the zinc. When the wire was wiped dry, it no longer caused contractions; but they could be always renewed by moistening the end of the wire in the fluids of the frog. This shows that my first conjecture, concerning the cause of the contractions effected by a zinc wire in contact with the nerve, was well founded: it shows also the great susceptibility of the animal fibre to chemical stimulus.

I repeated the experiments on some of the metals as before, but in place of the dilute acid,

a drop of pure water was used. The effects were seldom sensible.

Whoever considers the foregoing experiments will probably agree with me, that it is extraordinary, if the office of the wires be merely to act as conductors to restore the equilibrium between the plates of metal, that gold, silver, and platina, should fail altogether in conducting the discharge; while zinc should produce a powerful effect. If electricity were the cause of contractions, these different metals should be at least equal in efficacy, especially since we find that all of them could conduct well when chemical action was going forward.

Having thus proved that substances incapable of being chemically acted on by the animal fluids, produced no contractions, it was probable that charcoal would be equally inefficacious. I therefore procured insulated wires of platina, silver, copper, and zinc, and having bent them into the form of a discharging arc, I affixed to one end of each a slip of well burnt charcoal made from dense box-wood.

The platina and zinc apparatus was arranged, with the frog as usual resting on a plate of platina connected with the compound plate. The metallic end of the platina wire was applied to the zinc surface of the compound plate, and the charcoal end to the nerve. I was surprised to find that a contraction followed which, however, was feeble. With double plates of the other metals and arcs, the charcoal produced similar effects.

A little reflection showed that this result, far from opposing, seemed to strengthen my supposition, that chemical action was the cause of the convulsions. In the charring of wood, we know that a powerful chemical agent is produced, possessed of energetic affinities. This alkali, contained in every part of the charcoal, might easily be supposed to act on the salts or even on the animal matter of the frog, and thus produce contractions, as in any other case of chemical action. If this conception were well founded, it ought to happen that by frequent lixiviations, the charcoal should be deprived of its saline matter, and might then probably become inactive on the frog.

Several specimens of fine box-wood charcoal were therefore repeatedly boiled in water twice distilled at a temperature below 212°. They were taken out with caution, and placed in a platina crucible filled with pure silex. The crucible was heated to redness; and when cold, the pieces of charcoal were taken out, and subjected to the experiments as before. I now found that contractions were produced as strongly as ever. It was possible that the silex might contain a minute portion of saline matter which escaped elutriation. I therefore determined to ignite the next specimen in hydrogen.

Accordingly, after frequent boiling in pure water, some new pieces of charcoal were introduced into a tube of glass filled with hydrogen, and that end of the tube was heated as

much as it would bear without melting. Notwithstanding that every care was observed in taking out the pieces, and attaching them to the wires without collecting impurities, yet when applied to the nerve of an irritable frog, they produced contractions.

These results did not, however, convince me that there was no source of fallacy. From considering the effects of the second ignition of the charcoal, it became evident that the contractions might be accounted for in two ways. Either a quantity of alkali, which being in the interior of the charcoal, escaped the action of boiling water, and was volatilized in the subsequent ignition through the external portions: or, as alkali is the result of charring wood, so a new portion might be formed during the second ignition; an opinion further countenanced by the continual evolution of hydrogen, while charcoal is exposed to a strong heat.

Were either of these suppositions well founded, the cause of the power to produce contractions had been the second ignition of the charcoal, and this it was my object now to avoid. A new portion of charcoal was boiled in several portions of the purest water, and then exposed to the rays of the sun, which soon dried them. When these specimens were tried, I had the satisfaction to find, that not the smallest contraction was produced: not even when the charcoal was attached to a zinc wire. The trials were repeated upon different frogs,

some being of exquisite sensibility, but in no instance was there any motion. Indeed unless the irritability of the animal were great, even the common unwashed charcoal had no great effect.

The only objection to this experiment which occured to me was, that by the loss of its saline matter, the conducting power of the charcoal had been impaired. But this was removed by finding that, when the end of the charcoal was armed with a bit of zinc wire, so that the arc consisted of two zinc wires with an interruption of a bit of charcoal in the middle, contractions were produced as powerfully as if the arc were entirely of zinc. Or more simply, if the zinc wire armed with a bit of charcoal were used, with the charcoal end applied to the double plate, and the zinc end to the nerve, contractions equally resulted.

It then occurred that by restoring alkali to charcoal that had been boiled, it would be again capable of displaying its former properties. A piece of washed charcoal was boiled in solution of carbonate of potash, and after being dried at the fire, was attached to a platina wire. When this charcoal was applied, in the usual manner, to the nerve of a new frog, contractions followed.

In all these experiments, it must be observed, that every result did not present itself so immediately, nor with such little difficulty, as is here represented. I met with frequent dis-

appointments from the frogs; they often appeared to have been in a state of indisposition, and thus to have lost much of their irritability, which put me to the painful necessity of destroying numbers of these harmless creatures.

On the other hand, their excessive sensibility has often proved to me a source of continual mistake. In some cases, when the body was completely divided above the pelvis, if the ischiatic nerve were simply touched with a wire of any metal, with a bit of glass, with a finger, or in short with any thing, strong contractions ensued. In one case, the pelvis and lower extremities were separated from the trunk, the skin was taken off, and the limbs thus prepared were laid on my hand; contractions followed in such quick succession, that it was difficult to believe that the parts were no longer alive. In all cases, after the decapitation of the animal, if the trunk be divided from the pelvis, by cutting with a scissors across the lumbar vertebræ, violent contractions take place when the blades touch each branch of the ischiatic nerve. And it is far from being uncommon to see the limbs contract without any apparent cause, when lying on a table. From these facts it appears, that frogs immediately after death are not in a state fit for experiments, and that to avoid being misled, a certain period should elapse previously to their being operated on. For want of this caution, many inquirers have been led into error: and to this source we may probably refer

the mistakes that were made with regard to vegetable galvanic series, &c. In my experiments I have drawn no conclusions but what were warranted by invariable results; and I hope that those who may repeat them will judge of my accuracy by the same standard.

With regard to the experiment in which a plate of platina joined to a plate of zinc had been used, the frog being connected with the platina side, and platina wire, proceeding from the zinc side, being applied to the nerve, it might be objected that the zinc was in contact, at both surfaces, with platina, and that therefore, as Volta affirms, the equilibrium was not disturbed. With a view of obviating this objection, I repeated the experiment in the following manner. The frog was laid, as before, on a platina plate connected with the platina side of the double plate: from the zinc surface proceeded a zinc rod, to the end of which was attached about one tenth of an inch of capillary platina wire. This bit of platina wire was applied to the nerve, but produced no contractions; yet when the zinc end was tried, the platina end forming the connexion with the zinc plate, contractions took place. In this experiment, it could not be supposed that a particle of platina wire, not exceeding one eighth of a grain in weight, could counteract the electro-motive influence of a platina plate of four inches surface acting on a similar zinc plate. If that particle of wire possessed any counteracting electro-motive power, it should only affect an equal surface of

the opposite plate, and after this it should act merely as a passive conductor.

The evidence afforded by the preceding experiments was, that, in some cases, electricity, consistently with obeying its usual laws, could not have operated: in others, when the circumstances for bringing electricity into action were most perfect, that there were not the least manifestations of its presence: and in all cases, when the conducting wire was such as could be chemically acted on by the animal juices, that contractions were produced, and never else. It therefore became necessary to investigate more particularly the separate effects of electricity and chemical action on animals.

I erected an electrical column of 120 pairs of polished zinc and copper plates $\frac{3}{4}$ inch square, with dry paper interposed, the whole being set in a frame constructed of glass pillars. When one end was connected with the ground, and the other with a sensible gold leaf electrometer, in which the conducting slips were very near the leaves, a divergence of a quarter of an inch was produced. The negative conducting wire was connected with the muscles of an irritable frog; and the positive wire was applied to the nerve. No contractions ensued.

The frog was now removed from the electric column; its muscles were connected to a single copper wire, the nerve to one of zinc: every time the further ends of the dissimilar metals were brought in contact, violent contractions ensued.

I then erected a pile of 500 pairs of zinc and

copper plates, each containing on each of its surfaces five square inches: both metals were highly polished, and so flat as to lie in contact; and the pairs were separated by equal surfaces of dry paper. Glass pillars and other contrivances retained this massive metallic column in its vertical position; and the whole was sustained on a strong insulating stand. The electrical intensity of this pile was so great, that when one end was connected with the ground, and the other with Bennet's electrometer, the gold leaves struck the sides at the rate of sixty times in a minute, but they never struck more than eight or ten times without adhering to the foil. Sharp pointed wires from the extremities of the pile, when brought together by the points afforded sparks; or if placed on each side of a Leyden phial, afforded a charge which would give a sensible shock to the tongue, or could be discharged in the form of a spark.

A vigorous fowl was killed by cutting off the head; the sciatic nerve was immediately exposed and coated with foil: the muscles of the leg were also enveloped in foil, so as to be in direct contact. The negative wire of the pile of 500 was connected with the muscles, and the positive wire was repeatedly applied to the coating of the nerve, but not the least contraction was produced. Yet by applying the conducting wires, connected with a single pair of plates in which chemical action was going forward, very sensible

contractions were produced; and three pairs produced a strong effect.

A rabbit that had been just killed had its sciatic nerve, where it emerges from the lumbar vertebræ, divided and coated with foil. The leg was also laid open, and the sciatic nerve with the adjoining muscles were enveloped in some folds of the same foil. The conducting wires from the large column were applied to both pieces of foil, but not the smallest effect was produced. When the wires from a pile of three pairs, in which chemical action was going forward, were tried, they caused sensible contractions; and five pairs produced a strong effect.

The result of these experiments is striking. In these cases wherein the contact of the smallest possible wires with each other, and with the animal, would produce contractions, how inconceivably small must be the electricity evolved? Yet when the quantity of electricity was many thousand times greater, as in the case of the large pile, no effect was produced. How then is it possible to suppose that the contractions were owing to electricity?

I carried these experiments on the inefficiency of electricity still further. The sciatic nerve of a prepared frog was coated with foil, the crural muscles were also coated, and the limbs were laid across two insulating stands, in such a manner that the coated nerve lay upon one, and the coated muscle upon the other. A wire proceeding from the positive conductor of a

powerful Nairne's electrical machine was made to communicate with one of the coatings of the animal, and a wire from the negative conductor with the other. When the machine was put in action, a large quantity of electricity must have passed through the frog, and even in the narrow track of the nerve, yet there was not the smallest indication of contraction.

I made another experiment in which the quantity and rapidity of the electricity passing through the animal was much greater than in any of the preceding cases. A fowl, prepared as before, had its crural muscles connected, by means of a wire, with the outside of a Leyden jar capable of containing five gallons. The nerve was connected with another wire, by wrapping both in foil, and the other end of this wire was attached to a slender sharp needle. The jar being fully charged, the needle was rapidly moved, by a glass handle, towards the ball on the top of the stem, so that the point drew the whole charge from the jar, and transmitted it through the animal with the utmost rapidity; yet not the slightest contraction could be observed.

In these experiments, in which the common electric machine was used, it is true that if a very small quantity of electricity were suffered to strike on the nerve from a distance, and through the air, contractions followed. But I conceive that these contractions were not produced by the same causes as those occasioned by the contact of two dissimilar wires. If the feeble and suppositious

electricity of two bits of wire were sufficient to affect the animal, it should also be affected by the large quantity transmitted from the machine or jar, through the conducting wires in contact with the nerve and muscles. We are rather to consider the contractions effected by the striking of common electricity from a distance, in the same point of view as those produced so frequently by mechanical causes as stirring the nerve with glass, pulling it, &c. When such causes as the latter contract the muscles, it is no wonder that the passage of a subtile matter like electricity should do the same. But this mode of explaining the fact is only used for the present: I conceive, although this mechanical explanation may be applicable in some instances, that the contractions effected by common electricity are produced by causes far different, which I shall enter more fully on hereafter. In the experiments with common electricity, made by various philosophers, with a view to establish the identity of electricity and galvanism, the discharge must have been made through a portion of air. But that this discharge must have some other influence than the mere passage of so much electricity, is shown clearly by the foregoing experiments. An additional fact of the same tendency is the following. Let the limbs of a frog, with the sciatic nerves hanging, be held between the fingers, so that either nerve rests on the conductor of the electric machine, and forms the only communication with it; thus all the electricity

generated must pass through it. When the machine is put into action, the limbs act as a passive conductor, being not affected even in the smallest degree. Can sound philosophy admit as a cause that which when present fails in producing its attributed effects, and which when absent does not prevent their appearance?

Not less inexplicable than the phenomena of contractions, is the power of Voltaic piles in producing the shock, while electricity is supposed to be the cause. The following experiments present a difficulty on this head, not easily reconcileable to the received opinions.

A pile of 100 pairs of highly polished zinc and copper plates was erected; the metallic pairs were separated by squares of new colourless cassimere which had been repeatedly boiled in distilled water, to extract every thing that might act chemically on the metals: and they were finally moistened in distilled water. This pile was introduced under the receiver of an air pump, through the glass of which passed a conductor connected with the top of the pile within, the latter being sustained in a metal pan resting on the pump plate. The receiver, being exhausted, was filled with hydrogen, which was then withdrawn and a fresh portion of gas was admitted. The moisture condensed so rapidly on the receiver during the exhaustion, that it was with great difficulty it could be removed from that part of the glass through which the conductor

passed, and which should be dry for the success of the experiment. This was however effected by means of a red hot iron held at some distance. I now applied wet fingers to the pump plate, and to the conductor communicating with the top, but could not perceive the slightest shock.

A pile of an equal number of plates was also erected, but the cassimeres were moistened in dilute sulphuric acid. The shock afforded by this was considerable. Yet in both, the electrical intensity indicated by the electrometer was the same, as far as could be judged by this small number of plates. In Volta's experiments, under similar circumstances, the intensity was likewise the same; but his results have been called in question by Biot, who by the use of the electrical balance, found that the intensity varied with the interposed fluid. But admitting the fact, it can have so little influence that it may be here neglected. Or if it have any influence it is still more favourable; for Mr. Singer found that the electric intensity was strongest when water was the interposed fluid; and weaker when the fluid was dilute muriatic acid. Here then were two piles of equal numbers, and equal electric intensity, yet one gave a strong shock, the other none: how is this to be reconciled to the laws of electricity? Should it be supposed that the non-action of the water pile was owing to the incapacity of that fluid to conduct electricity, it is only necessary to state that if the polar wires of an active battery be immersed in

unconnected portions of water, a shock will be received when the fingers are immersed in the water, which could not happen were the progress of the electricity interrupted. Or should it be supposed that the quantity of electricity was greater in the case of the sulphuric acid pile, it may be answered that quantity has nothing to do in the case, and that it is intensity and not quantity which produces the shock. Thus if a few sparks be thrown from an electric machine into a very small Leyden phial, and ten times as many into a very large battery, it will be found that the phial gives a smart shock, and the battery little or none, although the latter contains ten times more electricity than the former. It need not be supposed in this case that the cause of the difference of the shock given by the battery is the retention of electricity by the large surface of glass after the discharge. The experiment succeeds as well if the battery have been previously charged and discharged, and therefore containing whatever portion the glass should retain. Another fact which proves that quantity of low intensity has nothing to do in producing the shock is, that if a large battery of jars be discharged through the body by touching the outside with one hand, and bringing a sharp fine needle suddenly near the discharging ball of the battery, there will be no shock, although it cannot be doubted that an immense quantity of electricity passed through the animal circuit.

A pile of thirty plates, excited by dilute nitric

acid, will not affect Bennet's electrometer, yet if the acid be tolerably strong, it will give a smart shock. How inconsistent with the known effects of electricity must it then be to imagine that this suppositious electricity, not sensible to so delicate an instrument, can produce so great an effect upon the organs of sensation and motion; when it is known that dense sparks in a continued stream may be passed through a person placed between the conductors of a common electric machine, without any such sensation being produced.

The explanation of the light and heat produced in galvanic experiments is liable to as many objections as any other part of the hypothesis. With a battery of 40 pairs of plates, each 18 inches square, the effect on the gold leaf electrometer is barely sensible; yet two pairs of these plates will ignite and fuse some inches of platina wire. It is inconceivable how such a feeble intensity of electricity could accomplish this; and it is quite incompatible with all experiments with common electricity. The followers of this hypothesis affirm that it is the large *quantity* present that causes the ignition and fusion. But hav ethey proved by experiment that quantity of electricity ever produces ignition or any other effect: and have they any other grounds for their opinion than inferences drawn from the hypothesis itself,—from the thing to be proved? If the large quantity of electricity evolved from two pairs of plates ignite a wire, it can only do so in consequence of passing

through so small a quantity of matter, and therefore becoming condensed. This is equivalent to an increase of intensity: for intensity of electricity means no more than a large quantity collected on a small surface. But if the wire be fused by the intensity of the electricity, or what is the same, by the condensation of a large quantity upon a small surface, how are we to account for the non-effect on the electrometer, for the leaves separate with the feeblest intensities?

The light which appears before the eyes, even although closed, when a plate of gold is brought in contact with a plate of zinc in the mouth, is not explained by the electrical hypothesis. Can the small quantity of electricity generated by two bits of metal produce such an effect upon the nerves connected with the eye, when a very large quantity of common electricity will produce no such effect?

The preceding facts are inexplicable while it is supposed that electricity is the agent in galvanic phenomena. But by admitting that they are the result of chemical action, we obtain a greater probability of an explanation: and the application of this principle to the general facts will be attempted in a subsequent division of this Essay. The followers of Volta, to avoid the necessity of allowing chemical action to be the cause of galvanic effects, assert that chemical menstrua owe their superior power to their being

better conductors, and to a few circumstances of less consideration. This is another grand question on which a great part of the claims of the Voltaic hypothesis depends: it is therefore necessary to give it an experimental investigation.

A small pile, composed of two pairs of platina and silver plates, with two pieces of cassimere freed from every thing soluble, and afterwards moistened in pure carbonate of potash, was connected in the usual manner, with the limbs of a newly prepared frog, the connexion being made by means of platina wires: this pile did not occasion the smallest muscular contraction. Now there are but three ways to account for this want of effect; either there was no electricity evolved by the association of silver and platina, or if there were, the alkaline solution did not conduct it, or this solution possessed an electro-motive power equal to the platina, and therefore counteracted its effect; but the latter is so improbable that for the present it may be neglected. To prove that electricity was evolved by these metals, the alkaline cloths were changed for others soaked in dilute nitrous acid: and on making the communication as before, strong contractions were produced. To prove that the electricity thus shown to have been evolved, could be conducted, at least in sufficient quantity, by the potash, I substituted zinc plates for those of silver, again employing the alkaline cloths: this combination also produced strong contractions.

I made analogous experiments on galvanic

decompositions. A *couronne des tasses* was arranged, composed of sixteen glasses containing dilute nitric acid, with arcs each composed of a piece of charcoal joined in an acute angle with one of copper, the charcoal end of each being in a different glass from its copper end. This arrangement gave tolerably strong shocks, and rapidly decomposed water: hence electricity must have been evolved by the association of copper and charcoal. In place of nitrous acid, the glasses were now filled with neutral solution of nitrate of copper, and the same arcs were immersed as before. This arrangement possessed no energy, although the former proved that electricity was produced. According to the second objection before mentioned, it may be said that the nitrate was a bad conductor; but this was obviated by substituting, for the copper and charcoal, arcs made of copper and zinc, immersed in the nitrate. So well did the solution now conduct that from the same surface and number of arcs, shocks and sparks were produced, water was rapidly decomposed, and copper was precipitated in all the glasses.

A pile of twenty pairs of copper and lead was constructed, using very dilute sulphuric acid to moisten the cloths. This apparatus gave no shock; the negative wire did not emit a stream of bubbles as is usual when immersed in water, but gas in small quantities attached itself to it, and occasionally a bubble rose to the surface. When trial was made with dilute muriatic acid,

the formation of gas nearly ceased after a minute: yet the same cloths when used with an equal number of zinc and copper plates, decomposed water rapidly. We know that these acids are the best of the fluid conductors; and that electricity was evolved by the association of lead and copper, was proved by using cloths moistened in dilute nitrous acid; for in this case, shocks were obtained, and water was readily decomposed: hence it remains to be answered by the adverse side, why muriatic and sulphuric acids were so far inactive.

A pile of twenty silver coins, and discs of platina, with cloths moistened in solution of carbonate of potash, produced no effect: but when cloths soaked in dilute nitrous acid were used, the pile became active.

In all the preceding cases, it appears that the combinations of the metals were adequate to produce electricity, and actually did so; it appears also that the inefficacious fluids were capable of conducting the quantity produced. The question therefore is, why were these combinations inactive with the specified fluids? The adherents to the electrical hypothesis will answer, that the inefficacious fluids possessed an electro-motive power which counteracted that of one of the associated metals: but we shall see how far this opinion is warranted by facts.

Beginning with the combination of charcoal, copper, and nitrate of copper, which has been shown to possess no energy, it must be observed

that the power of charcoal and copper to disturb the equilibrium in a considerable degree, has been proved by the instance in which dilute nitric acid was used. That the nitrate of copper was a good conductor was also proved by its conducting the large quantity of electricity evolved by the arrangement of it with zinc and copper in the *couronne des tasses*. Since this solution of copper produced no effect, the followers of Volta will say that it possessed an electro-motive power of the same nature as that of charcoal: and therefore the copper being in contact with two substances of similar nature, or, as in effect might be said, with charcoal on each side, a counteracting force was applied. But since *no* electricity was manifested, I have by this reasoning a right to conclude that the electro-motive powers of the substances on each side were precisely *equal* and *balanced*. If this be so, it should happen that copper would be equally active with either charcoal or solution of nitrate of copper: and to put the truth of this to the test of experiment, I proceeded in the following manner.

An arrangement of forty glasses, containing alternately solution of nitrate of copper and water, was made. Slips of sheet copper, about seven inches long and one in breadth, were bent in the middle to an angle of 45°, and one leg of each was immersed in a glass containing the nitrate, while the other was in the next glass which contained water, the glasses being connected in pairs by bits of moistened cloth. The

evolution of electricity, if any, was to take place between the copper and the nitrate, and the water was to act merely as a conductor possessing, as Volta allows, no electro-motive power. In short the arrangement was similar to that of tin plates, in contact at one side with water, and at the other with nitrous acid; a combination which Sir H. Davy found to possess great activity. When wires from the extreme glasses were immersed in water, at the distance of only $\frac{1}{10}$th inch from each other, there was not the smallest effect produced: nor did the arrangement afford a shock or taste.

The glasses were now alternately filled with solution of carbonate of potash and water. To avoid the expense of silver plates, silver coins were used; two, connected by silver wire bent in the middle, forming each arc, 20 pairs in all being employed. These were disposed in such a manner that one coin of each pair was in the alkali and the other in the water of the next glass, and the glasses were connected in pairs by bits of moistened cloth. This arrangement had no more power than the preceding.

Thus it has been proved that the cause of the non-action of those arrangements, in which the metals were capable of evolving electricity and the fluids of conducting it, could not have been the counteracting electro-motive power of these fluids. So that the electrical hypothesis in its present state, seems quite insufficient to explain these phenomena. From these experiments, it is

also evident that in no case, unless where chemical action is going forward, are galvanic effects manifested. It is however admitted that electricity may be produced independently of chemical action: but it is attempted to be proved that this electricity is not the cause of galvanic phenomena; and accordingly, it has been shown to be inefficacious in producing muscular contractions, shocks, decompositions, and ignitions. Considerations will be hereafter adduced which render it probable that electricity, in these cases, is no more than subaltern, directed by circumstances that shall be fully stated.

Beside the difficulties already adverted to, it is hard to conceive how the decomposition of bodies can be effected with so much ease by electricity so feeble in intensity and trifling in quantity, as in most cases it must be. Three pairs of plates, with cloths soaked in nitrous acid, will readily decompose water. But piles of much greater extent, in which chemical action is not going forward, possess no such power. De Luc's electrical column, which strongly affects the electrometer; Mr. Singer's immense combination, which causes the divergence even of large pith balls, which communicates such a charge to a jar as will give a considerable shock; and my column of 500 pairs of plates, each containing five square inches of surface, possess no chemical powers. The reason assigned by Sir H. Davy for this non-action is, that the quantity of electricity is not sufficiently great, but the intensity of the

small quantity existing is sufficient to affect the electrometer, and to act through a plate of air. But in the abovementioned columns, when attraction and repulsion are strongly manifested on heavy bodies, when even dense sparks may be obtained, and jars charged so as to be capable of giving strong shocks, would any one suppose that the quantity of electricity is less than what is produced from three small pairs of plates, which give neither sparks, nor shocks, nor charge jars, nor affect the electrometer, yet will easily decompose water. As to Sir H. Davy's explanation, I see no reason for supposing the quantity of electricity to be small: the contrary is even proved by its intensity. In my electrical column of large plates, the surface electrified is extensive; if a small quantity were diffused over this, the intensity must be low: but since the intensity is high, it proves that the quantity is adequate to produce that intensity upon so large a surface. Thus, the inferences drawn from these columns which produce no chemical effects, operate with full force, since their non-action can not be explained on the electrical hypothesis.

Of the facts which favour the identity of galvanism and electricity, those dicovered by Wollaston and Davy are amongst the most convincing: so much indeed that they have been looked on as decisive on this important question, and they have in a manner put an end to discussion. It is therefore necessary to investigate what degree of evidence they really afford.

Dr. Wollaston found that by passing electricity in a small stream through water, it was decomposed into its elements : but whether one wire or two were employed, each gave off both gases together. That electricity decomposes water, and other substances, is a fact long known. Dr. Wollaston's method is an imitation of the galvanic process ; but he with great candour confesses that the imitation is not perfect. There are, however, many objections to the inference that the agent in the galvanic and electric decomposition is the same. First, it does not appear from the mere circumstance of the decomposition in both cases, that we can presume the agent to be the same. Heat is a more universal agent in decomposition, and we should be bound by the same reasoning to conclude that heat is identical with the cause of galvanic phenomena. Second, three pairs of plates will decompose water with great ease : how small must be the quantity of electricity set in motion by them. Yet the immense quantity produced by a powerful Nairne's machine, and this even condensed upon a wire as thin as a human hair, was required to effect the decomposition by Dr. Wollaston's method. Third, the gases are evolved together, a fact which alone proves that it is a mere decomposition of the ordinary kind : for the essential difference of the galvanic process is, that the gases are evolved at a distance from each other ; and this being not present, we are not permitted to establish any specific distinction. These considerations seem greatly to ob-

viate the necessity of the inferences that have been drawn from the foregoing experiment.

Sir H. Davy conceives that he has removed the objection with regard to the evolution of both gases together, by immersing a fine platina point in water, and hanging moist fibres of cotton, proceeding from the water, over the rim of the containing vessel, so as to dissipate the electricity thrown in by the point. If the wire conveyed positive electricity, he obtained oxygen; if negative he obtained hydrogen: the alternate products, he supposed to be evolved at the points of dissipation of the electricity. This experiment I consider objectionable: and it does not appear to present any new result. When oxygen was given off by the point, he conceives the hydrogen to be evolved by the cotton ends. But where could hydrogen have been produced if not at the platina point? We are not to suppose that the cotton fibre in the water could act like the negative wire of a Voltaic pile, and evolve hydrogen. We know that no bodies but metals and charcoal possess this power; and even with the pile, two wires are indispensable to the evolution of the two gases from water. So that the hydrogen also must have been produced at the platina point, that is, both gases were evolved together as in Dr. Wollaston's experiment. As to why the hydrogen did not appear, there is no proof that it was dissipated by the cotton; and it is more probable that it entered into a combination, as will presently appear.

In another experiment, Sir H. Davy, by transmitting common electricity through three or four grains of a saline solution in Dr. Wollaston's method, obtained acid at the positive side, and alkali at the negative side, in the same manner as if the solution had been submitted to the action of the pile. But there is reason to think that acid and alkali might have been *produced* in this experiment, and that they did not proceed from the decomposition of the saline solution. Sir H. Davy has not stated in what manner he convinced himself that the minute and scarcely appreciable quantity of alkali, which could be produced by the feeble action of common electricity on *three or four grains* of solution of sulphate of potash, was the real basis of that salt. We are therefore at liberty to suppose that he satisfied himself of the presence of an alkali by its effects on vegetable colours, and it was very natural to conclude that it was potash; and in like manner that the acid was sulphuric. It is, however, easy to account for the appearance of acid and alkali, without supposing the salt to have been decomposed. We know that water must have been decomposed; the oxygen at one side might unite to the azote of the atmospheric air continually absorbing by the water of the solution, giving rise to nitrous acid: the hydrogen by uniting also to azote might form ammonia; and this supposition explains the non-appearance of the hydrogen in Sir H. Davy's experiment on the

decomposition of water. But in electrical experiments, the change of vegetable colours is a bad test of acids and alkalies. By transmitting sparks over a piece of perfectly dry litmus paper, I found it reddened in a short time: and that this redness could not be produced by any acid liberated by the electricity from a combination, is manifest from this, that none was present, the litmus paper having been carefully prepared. And what bears more forcibly on the question is, that a stream of negative electricity thrown on the paper also reddened it: nor could I by any means restore the colour of litmus paper that had been reddened by the smallest possible quantity of distilled vinegar. Now since *dry* vegetable colour could be reddened by *either* kind of electricity, can it be supposed that this was owing to any process similar to the galvanic: and if it was not in this case, can we in the other case consider change of colour as an infallible indication of the presence of acids and alkalies, wherever electricity is concerned? Much less can we consider it a proof, in the experiments alluded to, that the sulphate of potash was decomposed.

By transmitting the electricity of 100 turns of an eight inch cylinder, by means of silver wires $\frac{1}{120}$th inch diameter, through solution of sulphate of copper, Dr. Wollaston found the section of the negative wire coated with copper. In endeavouring to repeat this experiment, I used a new Nairne's machine with an eight inch cylinder; but after 3000 turns, I found no other change on the silver

wire than a slight blackness: my failure was, no doubt, to be attributed to my not having known the proper degree of dilution necessary for the solution. Yet this unsuccessful experiment affords an argument. If a bit of silver, in contact with a bit of iron, be immersed in solution of copper, the silver becomes coated with copper. If any electricity be evolved by the *contact* of these bits of metal, how minute must it be, since it is not discoverable by the most sensible electrometer: yet this problematical quantity is capable of effecting what the large quantity, afforded by 100 turns of an eight inch cylinder, could barely accomplish; and which, in a less favourable trial, was not accomplished by 3000 turns. Further, in the case of the bits of metal, a large surface of silver was coated, whereas in the case of the wires, no more than a mere point was affected. I therefore consider this experiment as rather proving that the electrical and galvanic decomposition of the salt are of a perfectly different nature: and it is much more probable to suppose, that the reduction of the copper was owing to nascent hydrogen, produced by the necessary decomposition of the water.

It now remains to make some observations on the electricity which galvanic series exhibit, and which have given rise to the opinion that electricity is the agent in galvanic phenomena. The Voltaic pile has been found to manifest attraction and repulsion: and these forces are amenable to the same laws as electricity. It has been

also found that the pile will communicate a charge to a battery of jars; that this charge may be drawn out in the form of a spark; and that, if passed through the body, a shock is occasioned similar to that of common electricity. These phenomena may easily happen, for electricity is really present. When a battery of jars is placed in the circuit of the pile, it becomes charged with electricity to the same intensity as the pile, and the shock which the battery communicates is in proportion to the charge. The shock given by the pile is owing to a different cause: it is not produced by electricity, but by means which it would be premature in this place to detail: indeed the sensation is perfectly different. I have already adduced reasons for the opinion that the shock of the pile is not electrical. The intensity of the pile so low, unless the plates be numerous, as not to affect the electrometer, is scarcely to be reconciled with the fact, that if the human body, insulated, be placed in the circuit of the most powerful electrical machine, provided the contact with the conductors be uninterrupted, the slightest sensation will not be perceived during its action. In this case, the quantity is immense, and certainly much greater than even a Voltaist could suppose to be circulated by twenty pairs of two inch plates, which when well excited, will give a tolerably strong shock. Beside, it has been shown that quantity does not operate without intensity, and if this be so, an ordinary pile could not give an electric shock. It was ascer-

tained by Van Marum and Pfaff, that a very large electrical battery, charged by a pile, gave but one half the shock which the pile itself afforded. This exactly accords with the view above stated. The accordance is still greater than this, for I have myself experienced the shock of a large battery charged by a Voltaic series of 1000 pairs of new plates. The shock was pungent, but trifling when compared with that of a small jar highly charged: whereas an instantaneous contact with the series of 1000 pairs, even by means of hands scarcely moist, produced a most intolerable sensation, and threw my whole body into spasm. Now it is evident that, were electricity the agent, the pile ought to give no greater a shock than the battery during an equal time of contact. Or, if there were any difference, the pile ought to give a less shock, because the body could not receive the electricity from the pile in such a rapid stream as the coatings and wires of the battery: the latter should therefore receive more in a given time, and pour it out more rapidly through the body. On the other hand, if the battery be charged by a pile consisting of a great number of plates, in which dry paper is the interposed substance, it will receive a strong charge, and this will give a proportionate shock; yet the pile which produced this effect on the battery is not capable of giving any shock by itself.* The same thing will happen if, be-

* Mr. Singer's column of 20,000 pairs charged a jar so as to give a strong

tween the plates, fluids be interposed which do not, or scarcely, act chemically on the pile: such as pure water, various saline solutions, solution of potash, or other good conducting fluids which exert but a feeble chemical action.

It is now necessary briefly to recapitulate the topics which I have endeavoured to illustrate in this chapter. It is admitted that electricity is present in the pile; that it displays its effects in attractions and repulsions, in charging jars and batteries. These batteries, when discharged through the body, give a shock which is electric, but depends on a different cause from the power in the pile which produces the shock. It is attempted to be proved also, that the electricity acts no other part in galvanic arrangements. That the contractions in the limbs of animals do not depend on electricity evolved by metals, but on chemical action exerted on the limbs, is shown by the fact that when metallic electricty was really present in large quantity, and without chemical action, no contractions were produced: the same may be said of common electricity. Galvanic series have been adduced which evolve electricity, but being incapable of chemical action, they cause no contractions: yet when in these series, chemical action was suffered to take place, the

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shock, but he has not mentioned the effect of the column itself on the body. I conclude that there was nothing that could be called a shock, from experiments made with my own column of large plates, containing 500 pairs.

contractions were immediately produced. Arrangements of charcoal and non-oxydable metals acted in the same manner. While the charcoal was in its common state, containing alkali, it was capable of acting on the animal salts or fibre, and therefore produced contractions; but when freed from it by washing, it possessed no such power.

Galvanic arrangements have been also instanced in which no chemical action was going forward, and which therefore possessed no power of effecting decompositions: yet it was proved that, according to the hypothesis, electricity was produced by the associated metals: that the interposed fluids were good conductors: and that these fluids were not inefficacious on account of possessing an electromotive power equal to that of one of the metals. It has also been shown that the same metals, with fluids which could act on them chemically, produced all the galvanic phenomena with energy. Finally, some considerations have been adduced to evince that the experiments of Wollaston and Davy, on the decomposing powers of common electricity, do not establish the identity of the two agents, since the results of these gentlemen can be explained in a different manner.

It now only remains to remove certain objections to the chemical origin of galvanism, arising from experiments in which electrical appearances have been produced when there could not be any chemical action.

The most commonly urged instance is the contact of Volta's plates. But here it is evident, from what has been shown, that there is no valid objection, if we deny the identity of the two powers. Electricity is produced, but the power which produces galvanic phenomena is not; because there is no chemical action. The same may be applied to Davy's experiments on contact of metals with acids, alkalies, earths, &c. which prove nothing if we deny the identity. The vegetable pile of Baconio proves to have been a deception, originating probably in the irritability of some frogs which contract with the application of almost any thing. The contractions said to have been produced by magnets, and by galvanized wires no longer in the circuit, are all to be considered as mistakes originating in the same source of error. The animal pile of Lagrave, and the contractions produced on a frog by contact with the cervical muscles of an ox, present no difficulty. In these cases salt was used, and this, as we have een, powerfully acts on the animal fibre.

The secondary pile of Ritter, constructed of copper and water, is incapable of acting by itself; but acquires that faculty after being placed in the circuit of an active pile; and retains it after its removal. When water is exposed to galvanic agency, nitrous acid, oxymuriatic acid, and ammonia are produced at the respective poles: so that in this pile, the copper becomes in contact, at one side with acid, and at the other with alkali. That this is a very active combination,

I found by arranging a *couronne des tasses* of copper arcs and glasses filled alternately with dilute nitric acid, and ammonia. Thirteen such arcs not only decomposed water rapidly, but gave shocks. In Volta's gold and water pile, the case is the same; the copper of the alloy is acted on, as well as the gold, by the oxymuriatic acid, and this would happen more especially in a galvanic arrangement. The action of the secondary pile ceases however in a very short time.

Sir H. Davy's arrangements of charcoal and fluids may be also referred to chemical action. I found that a series of 10 arcs of charcoal, with glasses alternately filled with sulphuric acid and water, scarcely produced contractions in frogs; but with charcoal arcs from which every thing soluble had been extracted by boiling, there was no effect whatever. If the acid had been nitrous, the action would have been energetic, because this acid acts violently on charcoal. Even solution of soda, with charcoal, shows indications of galvanic effects; but between these bodies there is a slight chemical action. So that assent cannot be denied to the proposition that galvanism is never evolved but during the chemical action of fluids on conductors when arranged in contact, at one part, with each other, and at the other, with the menstruum.

PART III.

Statement of a New Hypothesis of Galvanism.

HAVING now attempted to show that the hypotheses hitherto advanced are insufficient to explain the phenomena of Galvanism, it remains to present a statement of those views which, indulging myself in the hope, I have considered less exceptionable. This is by far the most arduous part of the undertaking, and will no doubt require much excuse. In laying down my opinions, I found it difficult to conduct myself between two opposite errors; first, that of aiming at too minute an application of the general principles; second, that of leaving them too little connected with phenomena. Both errors are equally dangerous; in the latter case, the system must appear imperfect, limited, and inadequate; in the former, it must assume a purely hypothetical aspect, which although it for a moment impose on the imagination, must finally alarm the understanding, and banish confidence. I have therefore applied my opinions to the principal and authenticated facts; but those that were of minor importance I have left to be explained according to the fancy of those who may think the task worth the trouble. "In the explanation of phenomena,"

says a celebrated philosopher, "to which a great number of properties contribute, we must not expect to be able to determine all the causes which act, and which may lead to modifications of the results: the number and agreement of these results may, however, be sufficiently great to show the principles from which they are derived, particularly when they are established on general principles which have the advantage of connecting phenomena with these general principles, which appeared to be independent of them."*

CHAPTER I.

General Principles.

1. WHETHER or not attraction be one general principle identical throughout its various modifications, is a question which, in the present state of knowledge, cannot be decided. The desire of generalizing, and of simplifying the operations of Nature, has influenced many in admitting the identity: yet the sensible properties of the different forms of attraction are so greatly dissimilar, so independent of each other, that it is difficult to give assent to an opinion so feebly supported.

* Berthollet's Chemical Statics, 1. 264.

There are nevertheless certain properties in which each resembles the other: it is observable that the attraction possessed by one body is capable of being made to act on another, and of even being transferred to it: but although there is this general agreement, yet the particular circumstances of each are different. When a gravitating body is supported by a fluid, the former loses a certain portion of its weight, which the latter gains, and is therefore more influenced by the attraction of gravitation than before. When a piece of zinc is amalgamated with an equal bulk of mercury, the cohesion of the zinc is impaired, that of the mercury is increased, it is no longer fluid, but coheres strongly and permanently. Those who consider cohesion as a mere effort of affinity must still admit that it is the cohesion of the zinc which affects the mercury. A piece of iron applied to a magnet receives magnetism, but with this peculiarity that the magnet rather gains than loses power. An electrified body when applied to any unelectrified body communicates electricity, and agrees with gravitation and cohesion in losing a a proportionate quantity of its power. In all these cases the analogy prevails. Can we suppose that it extends no further, and that the only remaining attraction, that of the atoms of heterogeneous matter, is excluded? This is the first subject of inquiry; it is the foundation of the doctrine of galvanism which I am about to detail: and I hope to adduce such considerations as will

entitle me to arrange affinity within the limits of the general analogy of attraction.

2. If a piece of zinc and a piece of copper be immersed in dilute nitric acid, but not in contact, they effervesce and dissolve rapidly; and oxides of zinc and copper are found in the solution. Hence, these two metals have an affinity for the oxygen of nitrous acid.

If the zinc and copper be brought in contact, and immersed in the dilute acid, both appear to effervesce, but the zinc alone dissolves.

3. Previously to drawing any inference from these facts, it is necessary to consider the explanation that would be given of them, according to the doctrines at present received. It would be said that the two metals by contact become electrical, the zinc positive, the copper negative; that the fluid is decomposed in consequence; and that the copper refuses to dissolve, by assuming the electric state of the acid. The objections to this explanation are numerous and forcible.

It has been already shown (p. 159,) that metals while in contact have never yet been proved to possess difference of electric state, nor do they manifest it until separated. Therefore it is an assumption in opposition to the laws of electricity to suppose a disturbance of the equilibrium while they are in contact. A second argument is, that it is difficult, if not impossible, to conceive, how the suppositious electricity existing in these metals, or even the minute quantity generated by their separation, especi-

ally in a conducting fluid, could effect the decomposition of nitric acid; for a very large quantity, from a powerful Nairne's machine, may be passed through this acid without decomposing it. It were singular that an electricity not capable of affecting a sensible gold leaf electrometer should perform what a powerful stream of common electricity would fail in. And if this feeble and suppositious negative electricity of the copper could prevent its solution in the acid, which has a strong affinity to it, is it not incomprehensible that a piece of copper wire, conveying the utmost negative intensity of a powerful electric machine, dissolves with as much facility in that acid, as if there were no electricity in question. Another argument is, that when both metals are in the same state, whether positive or negative, the same effects follow.

A wire of copper and a wire of zinc were connected, and sealed in one end of a glass tube, so that the junction of the wires was in the middle of the tube. The copper wire passed through the sealing, and was connected with the positive conductor of a powerful Nairne's machine; dilute nitric acid was poured into the tube, while the cylinder was kept in brisk action. The zinc part in the tube dissolved as before; the copper part gave out gas, but did not dissolve. The same effects were produced, when the apparatus was in the negative state. In these experiments, both metals were similarly electrical, and intensely so, yet the result was the same; a fact

proving clearly that the non-solution of the copper does not depend on its negative power, that the solution of the zinc does not depend on its positive state, and that the attraction of the elements of the acid to the respective wires does not depend on the electricity of the metals. It is in vain to allege that the electricity transmitted from the machine was "too small in quantity to influence chemical changes;" the electricity produced by the contact of bits of wire must be infinitely smaller, and yet this quantity is the cause assigned for the appearances.

4. In endeavouring to form somewhat correct inferences from the first mentioned experiment (2*) with pieces of zinc and copper, we must divest our minds of every assumed principle which has heretofore been offered in explanation of that class of facts called galvanic. Let us not suppose that an electric or a galvanic fluid, produces this effect: and above all let us reject the supposition that the metals are in different states of electricity, and that attraction and repulsion of gases take place between them. Encumbered in our progress by such difficulties we should maintain a laborious march amidst interminable obstructions; we should at length sink into a quagmire of hypothesis, and for ever lose sight of the light of truth.

* The enclosed figures refer to paragraphs not pages.

Examining the above phenomena therefore in only a chemical point of view, and as we would have done before the idea of the agency of electricity was conceived, it appears from the first experiment that since zinc and copper dissolve in the acid, these metals have each an affinity for its oxygen, and that they combine with it. It appears also that when they are brought into contact, the copper no longer unites to oxygen. What is the inference from this? I think the simplest and most consonant to the established properties of matter is, since the copper in the first instance did dissolve by its affinity to oxygen, and since in the second it did not, that the usual affinity of copper to oxygen was suspended; that for the time it was virtually lost, inasmuch as it did not act.

5. Now since the copper does not exert its usual affinity to oxygen, let us see whether that affinity be really annihilated, or whether it can be found again.

I took two zinc wires of equal weight and thickness, each being an inch long. The end of one was hammered into a perforation made in the centre of a new copper coin, so that the wire stood upright on the copper: the other wire was inserted into a disc of copper, in a similar manner: and exactly the same length of wire was exposed in each case. Both these instruments were placed in a glass dish, the cork being cemented to the bottom: and dilute nitric acid

was poured in until it stood above the top of each wire. The zinc connected with the copper began instantly to effervesce, and discharged a torrent of gas: the other wire was comparatively very little affected. The gas was received in two small jars that had been filled in the acid, and were inverted over the discs both at the same time. In a few minutes, the jar placed over the zinc connected with copper contained eight or ten times more gas than that which stood in cork: and when the former zinc was entirely dissolved, the other remained somewhat thicker than an ordinary pin, and effervesced long after. These results were nearly the same in ten trials, and it seemed immaterial whether the acid were nitric, sulphuric, or muriatic.

In a former experiment (2) it was found that copper lost its affinity for oxygen, by contact with zinc; in the last, that the affinity of the zinc for oxygen was much increased by contact with copper. I think therefore that there is nothing overstrained in the inference that one has gained what the other lost, or in other words that the copper has transferred a portion of its affinity for oxygen to the zinc.

6. It is not with an intent of conveying any conjecture as to the nature of affinity that I make use of the word *transfer*. I do not pretend to say that affinity is distinct *matter* which may be transferred from one body to another: the word is used barely to express an effect, without

touching on the cause. It will answer every purpose as well, and will be more consonant to received opinions to suppose that affinity is a property of matter of variable intensity: that in the foregoing experiment its force is diminished in the copper, and increased in the zinc, in some manner out of the reach of our present knowledge. Hence, without implying an hypothesis, I may in common language say that the affinity is transferred; and I hope it may hereafter be understood that whatever may be the mode of expression, no other than the above sense is conveyed.

These singular facts can scarcely be explained otherwise than by admitting that affinity is transferred: it will be found that the principle is suggested by numerous other phenomena, and that it harmonizes, in a striking manner, with some curious properties of metals which have not hitherto been observed.

7. Having found that affinity is transferred in the case of zinc and copper, I wished to ascertain whether or not the same happened in all other cases of metallic contact. In all the following experiments, the dilute acid employed was of such strength as to enable it to act on both metals, when separate: and the solutions were examined with some care to ascertain what metals had dissolved. The following exhibits the result.

1. Copper and zinc in contact were immersed

in dilute nitric acid; the zinc, as has been already mentioned, dissolved alone.

2. Copper, iron, and dilute nitric acid. The iron alone dissolved.

3. Copper, lead, and nitric acid. Lead alone dissolved.

4. Copper, silver, and nitric acid. Copper alone dissolved.

5. Copper, tin, and nitric acid. Tin alone dissolved.

6. Copper, mercury, and nitric acid. Copper alone seemed to dissolve: but at length the result became uncertain as the metals amalgamated.*

7. Copper, bismuth, and nitric acid, in various degrees of dilution. In all cases, a solution of both metals was obtained. The copper dissolved in less quantity.

8. Iron, mercury, nitric acid. Iron alone dissolved.

9. Silver, zinc, nitric acid. Zinc alone dissolved.

10. Silver, lead, nitric acid. Lead alone dissolved.

11. Silver, tin, nitric acid. Tin alone dissolved.

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* This must have been owing to the precipitation of the mercury from its nitrate, and not to the direct union of the two metals.

12. Zinc, lead, nitric acid. Zinc alone dissolved.

13. Zinc, tin, nitric acid. Zinc alone dissolved.

14. Gold, zinc, nitro-muriatic acid. Zinc alone dissolved.

15. Gold, copper, nitro-muriatic acid. Copper alone dissolved.

16. Gold, iron, nitro-muriatic acid. Iron alone dissolved.

17. Platinum, zinc, nitro-muriatic acid. Zinc alone dissolved.

18. Platinum, copper, nitro-muriatic acid. Copper alone dissolved.

19. Palladium, zinc, nitro-muriatic acid. Zinc alone dissolved.

20. Cobalt, zinc, nitro-muriatic acid. Zinc alone dissolved.

21. Nickel, zinc, nitro-muriatic acid. Zinc alone dissolved.

22. Arsenic, zinc, nitric acid. Zinc alone dissolved.

8. As in all these cases, but one, it appeared that one of the metals lost its portion of affinity to oxygen, it was desirable to know if this portion had been, in all cases, transferred to the metal which dissolved. To ascertain this, the metals which were expected to dissolve were divided each into two portions of equal surfaces, weight

and brilliancy. One portion was connected with the metal which was expected to transfer its affinity, and was in this state exposed to the action of the acid: the other was presented to the acid by itself. The metals were placed under jars filled with and inverted in the proper acid: and the comparative trials were begun at the same instant of time. The acids made choice of were often such as could act upon one only of the metals: for this method, as will be seen hereafter, was much less liable to uncertainty. The results were as follow.

1. Copper and zinc in contact, gave out in a given period several times more gas than the zinc alone. The result was the same, whether the dilute acid were nitric, sulphuric, or muriatic.

2. Copper and iron, with dilute nitric or sulphuric acid. The action was very feeble and tedious: the compound piece, however, gave out more gas than the single.

3. Copper and lead, with dilute nitric acid. The compound metallic piece discharged gas much more rapidly than the other.

4. Copper and silver, with dilute nitric acid. The compound piece evolved more gas in a certain time than the single; but the difference was not so great as in other cases.

5. Copper and tin, with dilute nitric acid. The production of gas from the compound piece was much more rapid than from the other.

6. Copper and mercury were not tried, as a

former experiment showed that the metals amalgamated.

7. Copper and bismuth were not tried, as both metals dissolve.

8. Iron and mercury with somewhat dilute sulphuric acid. The result was the same as in other cases, the compound piece affording more gas.

9. Silver and zinc, with dilute sulphuric acid. The compound piece acted much more powerfully than the single metal.

10. Silver and lead, with dilute nitric acid. The compound piece afforded more gas, but the difference was not great.

11. Silver and tin, with dilute nitric acid. The compound piece evolved gas in much greater abundance than the other.

12. Zinc and lead, with dilute sulphuric acid. The compound piece discharged torrents of gas, and far more rapidly than the single. In this case, the gas seemed to proceed entirely from the zinc, the lead affording little or none.

13. Zinc and tin, with dilute sulphuric acid. The compound piece afforded more gas, but the difference, although satisfactory, was inconsiderable.

14. Gold and zinc, with dilute sulphuric acid. Here the gas given off from the compound piece was 6 or 7 times greater than that from the single.

15. Gold and copper, with dilute nitric acid. The compound piece afforded more gas.

16. Gold and iron, with dilute nitric acid. The compound piece evolved much more gas.

17. Platinum and zinc, with dilute sulphuric acid. The difference was great.

18. Platinum and copper, with dilute nitric acid. The result agreed with the others.

19. Palladium and zinc, with dilute sulphuric acid. Here the difference was very great.

20. Cobalt and zinc, with dilute sulphuric acid. Result somewhat less striking.

21. Nickel and zinc, with dilute sulphuric acid. Difference great.

22. Arsenic and zinc, with dilute sulphuric acid. Difference sufficiently great.*

The first, fifth, eleventh, twelfth, fourteenth, seventeenth, nineteenth, and twenty-first experiments were particularly striking, but all were sufficiently so.

9. In these cases, it appears that affinity was transferred from one metal to another, so as to increase the ordinary action of the latter on an acid. It is even possible by this transfer to give an affinity to a metal which it does not naturally possess, at least not in sufficient force to attract one of the component parts from a compound. Thus if copper be immersed in somewhat dilute

* Ritter made some experiments of this kind, but with a perfectly different view.

muriatic acid, it is scarcely acted on, but if the copper be in contact with platinum, the latter gives out hydrogen, and the former blackens, dissolves in small quantity, and may be detected by ammonia. If pure polished lead be immersed in muriatic acid, it remains for a length of time untarnished. But if, while still in the acid, it be touched by platinum, both metals effervesce, the lead instantly blackens in a remarkable manner, and slowly dissolves. Or if a little muriatic acid be poured on a bright plate of lead, no change takes place; but the instant that the end of a wire of platina is applied, the lead blackens in the form of a star round the part, and both discharge gas. In the same manner if fine silver wire be rolled round a cylinder of platina, and exposed to strong muriatic acid, the silver will, in about 10 days, be dissolved, although without the platina it would remain unaltered.

10. From all the foregoing facts, it therefore appears that the principle of transferred affinity is general; and whatever may be the attraction of any metal to oxygen, that it is always either diminished or increased by the contact of another metal: the more oxydable metal acquiring the increase, and the less oxydable suffering the diminution. I hope it will be remembered that no other meaning is attached to the word *tranfer* than that the affinity of one metal is diminished, while the power thus diminished is found added to the other.

11. There are circumstances that limit the truth of all the foregoing conclusions, and therefore deserve particular attention. It is but reasonable to suppose that affinity is a property possessed by all matter, existing independently in each species; and when two bodies attract each other chemically, that we are not to consider the attraction of the second as a mere case of reaction produced by the action of the first, but as an absolute and independent force. This being so, if the affinity of two bodies be such as will effect a combination, and if the affinity of either be withdrawn, it ought to happen that the combination will be prevented. But if we increase the force of affinity in the other body, so as to compensate or rather to exceed the affinity so transferred, the combination ought to take place. Such is accordingly the fact. Zinc and copper, in contact, when presented to dilute nitric acid, do not both dissolve: for although the copper separately would attract oxygen from nitric acid, yet it loses this power by contact with zinc: and the affinity of the oxygen alone is not sufficient to overcome the cohesion of the copper. But when the force of affinity in the oxygen is increased by concentrating the nitric acid, the loss of affinity in the copper is counterbalanced, and both metals dissolve. The reason why concentrated acid has this superior efficacy seems to be, that a greater number of particles of oxygen are brought to act within a given space; the sum of the powers of which acting co-operately produces what is called

intensity of affinity. Thus, polished copper, which acts violently on nitric acid when the oxygenous particles are much approximated, produces no action when they are much separated by dilution.

This compensating power of acids is shown, in a striking degree, in some cases of metals in contact. The dilution of the acid must be as the feebleness of the affinity transferred from one metal to the other; otherwise both dissolve. Iron and copper, or lead and silver, require a very weak nitric acid to produce the desired effects: and bismuth and copper, be the acid ever so dilute, always dissolve together. Heat, as might be expected, has the same effect as concentration: on which account, it is often necessary, for the success of some of the above experiments, to have the containing vessels immersed in cold water.

12. Since then it appears that less oxydable metals transfer their affinities to those that are more oxydable, it becomes a question,—does not the transferring metal suffer a permanent diminution of its power of transferring affinity, in proportion as that affinity is received by the zinc? Or in other words, is the power of transferring affinity inexhaustible? The following experiments afford the answer.

A piece of sheet copper was brought in contact with a piece of sheet zinc, and both were immersed in dilute sulphuric acid. The copper, as usual, discharged gas, but the zinc alone dis-

dissolved. When this happened, another piece of zinc was put in place of the former; and this was repeated several times as fast as the pieces of zinc dissolved. The copper that had been thus treated was now connected with a new piece of zinc, and placed under a jar filled with and inverted in dilute sulphuric acid: a piece of zinc, equal in surface, weight, and polish, in contact with a new piece of copper that had been never before used, and equal to one half the surface of the other copper, was also placed under another jar of dilute sulphuric acid, and at the same moment. The new copper, and zinc connected with it, gave out torrents of gas, the old very little: and in a short time, the former copper, although but half the surface of the latter, had given out five or six times more gas. In another trial, the old copper was polished anew, but the result was the same.

I tried the same experiments with silver, and found that a piece which had been in contact with many bits of zinc during their solution, did not, in comparative trials, afford more than a fourth of the gas which a new piece of silver gave off. These trials were made a number of times, and always with the same success.

Since then affinity is a force transferable from one metal to another, and since one can be rendered permanently less capable of effecting the transfer, it would appear probable that this diminution of capability ought to be proportionate to the surface of copper compared with that of

zinc. To ascertain this, I attached three pieces of zinc, each being of the same surface and weight, to three pieces of copper, one of which was smaller than the zinc, one larger, and the third very large. These combinations were placed under three different jars of dilute sulphuric acid. In three minutes the quantity of gas was much greater in the jar that contained the largest copper, than in that which contained the middle size; but the quantity of the gas by no means increased in the ratio of the surface of the copper.

13. In all the experiments in which metals are made to transfer their affinity for oxygen to other metals, platina or gold will answer as well as, or better than copper: and on this circumstance an objection to the conclusion which I have drawn might possibly be grounded. If platina be thrown into sulphuric, nitric, or muriatic acid, it manifests no attraction to oxygen; there is no effervescence, nor does the metal dissolve. Hence it may be urged, since platina seems to possess no attraction to the oxygen of the acid, how can it transfer any to the zinc? The objection, as can be easily shown, is no more than apparent.

When a metal is presented to an acid, although it powerfully attract its oxygen, yet there are two principal forces opposing their union: first, the attraction of the oxygen to the other element of the acid; second, the attraction of the metallic particles to each other, or their cohesion.

Unless the opposing forces be overcome by the tendency to a new combination, no change takes place; such is the case when copper or lead is presented to sulphuric acid. When the opposing forces can be overcome, the new combination takes place; and zinc with nitric acid affords an instance. But because platinum does not decompose these acids, are we to conclude that it is for want of affinity to oxygen? The inference is obviously unnecessary; and well known facts convince us that it were untrue; for platinum and gold are easily oxydated by nitro-muriatic acid.

14. Thus, if my conclusions be admitted, we have arrived at the answer to the first question which was proposed, namely, whether or not chemical affinity come under the general analogy of attraction? whether or not it may be made to act on the similar power contained in other matter, in the same manner as has been observed to take place with regard to gravitation, cohesion, magnetism, and electricity? That this is so, must appear probable *à priori*; for it were singular that all other attractions should agree in one particular, and that this species alone should be excluded. Furthermore, the principle is rendered still more probable by the inquiries which have been just detailed; and my results, as far as I can judge, do not admit of any other explanation than that which I have offered. It is not to be supposed that this transfer merely relates to the affinity of metals for oxy-

gen; the principle must be general, and we shall find facts to be in favour of its universality. This is therefore the next subject of inquiry.

15. Cohesion is a force continually opposed to affinity. Although the latter is a force with which, no doubt, every species of matter is endowed, yet by the former power its operation is much obstructed. All metals possess affinity for oxygen, but as we have seen, they do not manifest it under every circumstance. The cohesion of manganese is easily overcome by its attraction to gaseous oxygen, that of tin is not: this in turn unites with the oxygen of nitrous acid, while the cohesion of platinum would cause it to remain unaltered in this fluid. When, therefore, bodies refuse to combine, we are not entitled to conclude that it is for want of affinity; we must rather suppose that all those substances which we find it difficult or impossible to unite, are acted on by some force which counteracts affinity. Amongst bodies thus circumstanced, we must consider hydrogen and metals, between which there must exist a mutual affinity; and that this is so we have the testimony of facts. The corroborations are both indirect and direct. Metals have an attraction for sulphur which we must suppose to be mutual, and therefore to exist equally on the side of the sulphur; this inflammable has an affinity for hydrogen which must therefore equally attract the sulphur: hence, since metals and hydrogen have

an attraction for the same intermediate body, it is probable that they attract each other. The direct corroboration of this supposition is, that there are many metals which even form combinations with hydrogen: thus we have both a solid and an aeriform combination of each of the three metals, potassium, tellurium, and arsenic, with hydrogen: and zinc and iron are also known to dissolve in this gas. In the same manner, carbon and many other substances have affinity for both oxygen and hydrogen, and form energetic combinations with each. On the whole, we may, without much hazard of error, admit as a fact that this species of attraction is universal, and that when it does not operate, it gives way to the predominant influence of some counteracting force.* The application of these observations will now appear.

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* On doit conclure delà, qui'il ne faut pas decider absolument qu'un corps n'a aucune affinitè avec un autre corps, parce qu'on ne connoit aucun moyen de les unir. Il paroit certain, au contraire, que tous les corps de la nature ont les uns avec les autres un certain degré d'affinitè, de facilitè à s'unir, un certain degré d'adhérence lorsqu'ils sont unis & que par consequent il n'y a point de combinaisons qui soient réellement & absolument impossibles; mais que ce degré d'affinité varie, suivant les differens états où ces corps se trouvent.

Encyclopédie, Art. Affinité.

The force of cohesion offers a resistance to the energetic action which produces combinations: hence it must not be concluded, because a combination does not take place, that the two snbstances have no mutual affinity.

Berthollet, Essay on Chemical Stat. 1. 38.

16. A numerous collection of instances have already shown that when two metals, in contact, are exposed to an acid, one produces a change in the affinity of the other. Thus in the instance first adduced, copper transferred its affinity for oxygen to zinc, when the two metals were exposed, in contact, to the action of dilute nitric acid. That the power of effecting this change of affinity should be confined to one of the metals would not correspond with the universality of the operations of Nature: we must rather suppose that the other metal would also effect some change on its associate; and this supposition is not only probable in itself, but stands supported by facts. Witness the following experiment, in which both changes are strikingly manifested.

Let a copper wire be sealed in one end of a glass tube, and a wire of platina in another: and let the ends of the wires pass out of the tubes at the sealing, so as to admit of their being connected when necessary. The tubes are then to be filled with muriatic acid, and inverted in a vessel of the same fluid. While the wires at top are unconnected, no change whatever takes place. But when the connection is established, the platina gives out hydrogen, and the copper dissolves, without evolving a bubble of gas. From this it appears that a mutual change in the affinities has taken place by the contact. The platina has transferred so much affinity for oxygen, to the

copper, that the latter unites to the oxygen of the water in the acid, which it could not do under common circumstances. The hydrogen which ought to be evolved at the copper, where the oxygen was absorbed, appears at a great distance, where it never could have been attracted to, but for some action exerted on that metal by the contact of copper.*

17. From these considerations and facts, there appears reason to believe that metals have affinity both for oxygen and hydrogen; and when two different metals are brought in contact, and presented to fluids which can satisfy these affinities, that the distribution of these forces is altered; one acquiring from the other an increase of affinity for oxygen; the other acquiring from the former an increase of affinity for hydrogen. This view is not opposed by the non-union of hydrogen and copper; the hydrogen is attracted to the metal; but as it cannot unite with it for reasons already stated (13, 15), it is

* I feel that the orthodox chemist will be startled at the operation of affinity at a sensible distance, which is here apparently implied. But it is only apparently: it will be shown, in explaining the decomposition of water, that the affinities only operate at the place of contact: and it may be here observed that distance need not be implied, inasmuch as there is perfect continuity between the wires, although through the medium of a fluid. Were it necessary, the operation of affinity at a sensible distance should not be rejected, because not hitherto acknowledged: this might be a property of transferred, although not of common affinity.

evolved : but instances wherein the union effectually takes place will be presently detailed.

18. A large plate of arsenic was connected with an equal plate of zinc, and exposed to the action of dilute sulphuric acid. As the acid became saturated, small portions of concentrated acid were, at intervals, added, so as to continue a slow action. After some days, the black powder which formed was collected on a filter and dried. This powder, which Doctor Thompson found to be a mixture of lead and copper, existing as impurities in the zinc of commerce, contained a minute quantity of hydruret of arsenic; for when heated very gently, I could plainly perceive the peculiar odour of that substance. The following is an easier mode of obtaining a similar result. Let a plate of zinc and a plate of arsenic, in contact, be exposed to dilute sulphuric or muriatic acid, during a few minutes. If now taken out, they will be found to smell strongly of arseniuretted hydrogen.

In both cases, therefore, arsenic and hydrogen had united, although under ordinary circumstances, no such union takes place; not even when these metals are in contact, unless the place of contact be also immersed in the menstruum, as appears by the following. A piece of arsenic was suspended in a small bell glass, by a capillary silver wire, which passed out through the sealing of the bell. A piece of zinc was similarly hung in another. Both bells were filled

with and inverted in dilute sulphuric acid, and the wires above the glass were connected. Both metals gave out hydrogen. When the bells were full, that which contained the arsenic was examined, but it had not the slightest smell of arsenic. Thus because the metals were not in direct contact in the fluid, the combination of arsenic and hydrogen was not effected, although the metal was exposed to the gas in its nascent state, and under circumstances as favourable to combination as in the other case, were the mere *natural* affinity of these substances the cause of their union.

It therefore appears that by the contact of the two metals, the zinc transferred to the arsenic an increase of affinity for hydrogen, and that by this increase the union was effected: and it appears that, at the same time, the arsenic transferred an increase of affinity for oxygen to the zinc, by which these substances combined more rapidly. The solid combinations of arsenic and tellurium with hydrogen, formed by making these metals convey the negative influence of a galvanic series, will be found strongly to confirm these views.

19. To suppose that the mutual interchange of affinities, by the contact of dissimilar metals, respects oxygen and hydrogen only, were to take a confined survey of the operations of this force. It is much more natural to suppose that all bodies will obey the same attractive power,

whether or not this be sufficiently great to overcome cohesion. The following experiments show that this is so.

A piece of sheet zinc, an inch long and half an inch wide, was connected to an equal piece of sheet copper, so that the pieces were $\frac{3}{16}$ths of an inch asunder, and communicated only by a small band of copper. On the inner surface of the zinc, a bit of litmus paper was fixed by neat tying with silk, and on the inner surface of the copper, was fixed a bit of litmus paper reddened with exceedingly dilute distilled vinegar. This small apparatus was immersed in solution of muriate of soda. In some hours, the papers were carefully detached and dried: the blue was reddened, and the red was rendered blue. The soda, like hydrogen, had been therefore attracted to the copper; and the acid, like oxygen, to the zinc: but the affinity of the soda was too weak to overcome the cohesion of the copper. The same effect may be produced with other neutral salts. The change of colour takes place principally where the paper is in close contact with the metal by means of the ligatures: but the vegetable colours must be in a state of great sensibility, and carefully fixed in the paper.

These results may be also obtained with metallic salts. If a slip of silver and a piece of copper, joined end to end, be exposed to the action of very dilute nitric acid, the copper alone dissolves; but as the process advances, the silver being thrown into the peculiar state of af-

finity under consideration, decomposes the nitrate of copper formed, attracts the dissolved particles of copper, and acquires a coating of them, while the zinc having its affinity for oxygen increased, dissolves with increased force. The following is a stronger instance.

If copper, in contact with zinc, be exposed to the action of dilute sulphuric acid, the zinc dissolves: but the copper acquires an increase of the opposite affinity, and if time be allowed, the copper exerts its affinity to the dissolved zinc so powerfully that it not only attracts it, but chemically combines with it, and acquires a strong covering of zinc, which cannot be detached unless by scraping with a cutting instrument, or by exposing it to an acid.

From all the facts with which we are now acquainted, it appears that the contact of dissimilar metals produces a remarkable change on their natural affinities. One acquires an increased affinity for oxygen and acids, so as to dissolve with increased force; or if it were incapable, under existing circumstances, of uniting with oxygen before, it during contact acquires that power. The other metal acquires an increase of affinity for other bodies: it attracts hydrogen, and when the case permits, even unites with it, although before contact it refused to do so: it attracts also alkalies, earths, and metals, in the latter case so strongly as to overcome cohesion, and to unite, although without contact no such effect would have been produced. Is it not therefore

allowable to infer, since metals separately do not produce these effects, and since they do when in contact, that contact is the cause, and that it operates by increasing the natural affinities ?

20. In the foregoing experiments, it is also to be observed that the metals in contact did not manifest an indiscriminate affinity for every kind of matter; but that oxygen and acids appeared at the zinc, and hydrogen, alkalies, earths, and metals, at the copper. This lays the foundation, in my trials, of a division of these bodies into two classes: but the division has been already more extensively established by the experiments in Galvanism made within the last ten years. I shall offer a few observations on this head, tending to connect this division with the foregoing principles.

That there are two states of bodies with respect to affinity, does not *primâ facie* appear improbable, since some other attractive forces of nature are well known to be similarly circumstanced. Magnetism and Electricity manifest two different states which cannot be satisfied indiscriminately by bodies in any state, but invariably exert their attraction to bodies of one state only: and that the same holds with regard to affinity, appears from the following. Let a galvanic series, composed of zinc and copper, terminate at the zinc side in a wire of zinc, and at the copper side in a wire of tellurium. The affinities of these wires are now in a state of in-

creased intensity: the zinc wire will unite forcibly to oxygen, and the tellurium will unite and form a solid combination with hydrogen. Why does not the zinc unite as well with hydrogen, and the tellurium with oxygen? The most probable reason is, that the zinc is not then possessed of that kind of affinity which hydrogen could saturate; nor the tellurium of that kind which could be satisfied by oxygen. This is at once establishing a difference between the state of affinity of oxygen and hydrogen; and as the principle holds in all other cases, it becomes general. Thus if the wires of a galvanic series be immersed in solution of mercury, the zinc end wire attracts oxygen, the other attracts and unites with mercury. If the fluid be phosphoric or sulphuric acid, a metallic phosphuret or sulphuret is found at the copper side, and the zinc side oxydizes. All these instances compel us to admit that the two affinities transferred do not attract all bodies equally; that they have really a different preference of attraction; and that they cannot be satisfied by the indiscriminate affinity of any other bodies. If this be admitted, assent can scarcely be denied to the proposition that chemical attraction, like the magnetic and electric, is attached to bodies in two different states, and therefore that all bodies may be divided into two grand classes, each being determined by the peculiar state of affinity which it possesses. To this general conclusion it will be no objection

to instance the combination which takes place between bodies of the same class. It is not to be supposed that bodies comprised under the same class are repulsive of each other, as is the case in electric attraction: on the contrary, they must be attractive; and the division into two classes must be only understood to relate to what fact itself demonstrates, the attraction of every body to one side or the other of a series affected by transferred affinity.

21. Hitherto, in mentioning the result of transferred affinity, I have been confined to speaking particularly of the bodies attracted. The above division into classes permits the law of transferred affinity to be given in its most abstract and therefore general form, namely, that when two different metals are brought into contact, an interchange of their natural affinites takes place, one acquiring an increased affinity for one class of bodies, the other acquiring an increase of affinity for the opposite class; and the affinity which one metal gains is derived from the other, which consequently is found proportionately deficient.

According to this view of the subject, we must suppose that affinity is a general property of matter, that all bodies attract each other chemically, that metals consequently have an affinity for all bodies, but that by their contact, they interchange their natural attractions, and that each has thus ac-

quired a predominating affinity for one class of bodies in preference to the other class.

22. With regard to transferred affinity, we have hitherto only examined the circumstances relating to the metals in contact: there are others, relating to the fluid, which require consideration. When a piece of copper and another of zinc are brought into contact, the usual transfer of affinities takes place, as much as the metals permit; and this is permanent. If in this state, the metals be merely exposed to air, the zinc will be found to oxydize slowly, but much more rapidly than without the copper. If the two metals in contact be immersed in water, the copper slowly gives out hydrogen, and the zinc oxydizes more rapidly than in air, because there is a more condensed supply of oxygen. The transfer in this case is more changeable than in the former case, because the affinities transferred are continually satisfied by the union of the elements of the water with the metals, and therefore a new transfer is slowly but continually taking place. If the water be acidulated, the transfer proceeds in rapid succession, because the affinities can be satisfied as fast as transferred: in this case all the effects are intense. To this circumstance is to be attributed the superior efficacy of acids in all galvanic phenomena.

23. If a metallic arc, half copper half zinc,

have one leg only (suppose that of copper) immersed in water, the affinities cannot operate, although they are transferred; for notwithstanding that the copper attracts the hydrogen of the water, yet this gas is attracted more forcibly by the oxygen with which it is united, so that no change ensues. If the whole arc be immersed, the case is different, for the copper attracts hydrogen, and the zinc oxygen; therefore as the two forces act co-operately, the water is decomposed. The same happens if the copper leg alone be immersed in dilute sulphuric, muriatic, or nitric acid; except that in the last case, although the copper cannot act by its transferred affinity, yet it does by its natural power.

24. If the arc of copper and zinc have one leg in one vessel of water or acid, and the other in a different vessel, the transferred affinities cannot act; for although both metals are immersed, yet they are each in the situation of the arc (23.) of which one leg only is in the fluid. The portions of water are different, and therefore the copper may attract the hydrogen, but without effect; for this gas is more strongly attracted by its associated oxygen, to which the zinc exerts no attraction, being in another vessel. The same reasoning may be applied to the zinc leg immersed in the other portion of water.

From all these facts, it appears that in order to obtain an *effective* transfer of affinities, two dif-

ferent metals must be in contact with *each other* and with the *same portion* of fluid.

25. Having thus attempted to elucidate those general principles of transferred affinity, it remains to apply them to the internal action of the galvanic series; the application appears easy and natural.

26. It is sufficiently evident that the effects of a galvanic series are no more than the sum of the effects of the members of the series, and mere inspection analyses the series into a number of metallic combinations, each composed of two dissimilar metals. From this, and from the considerations already adduced (22. 23. 24.), it plainly appears that the real element in galvanic series is the two heterogeneous metals in contact with each other, and with the same portion of fluid menstruum. The contact with each other need not be direct: intermediate substances succeed, as shall be presently noticed. It is first to be stated what the circumstances are for obtaining an accumulation of these transferred affinities.

A single pair of metals, in contact with each other, and with the same portion of fluid, is in itself a complete element: but by directly connecting a number of such elements, thus situated, we do not increase the power. The only method of obtaining this accumulation is, to break the junction of the metals, as will appear by the following.

Let a number of glasses be filled with dilute

sulphuric acid: let arcs of zinc and copper be arranged in the usual manner: and let the ends of the series remain unconnected. From every hypothesis hitherto offered, it is deducible that this must be the active state of the series: yet it is now destitute of all power, nothing taking place but the ordinary solution of the zinc. The connexion between this fact and the principles laid down (22. 23. 24.), is evident. The metals are in contact with each other, but not with the same portion of fluid, hence the transferred affinities cannot be effective. The metallic arcs are all independent of each other, and therefore there is no accumulation of tranferred affinity.

When the ends of the series are connected, and not until then, the galvanic action takes place. At this period, the zinc oxydizes more rapidly, the copper also gives out gas, but does not dissolve, even though dilute nitric acid had been employed. The cause of these changes must still be attributed to the contact of the metals, but not to their direct junction in the arc; for this we have seen to be insufficient, as the legs of the arcs would be then in different glasses of fluid, and as an arc so circumstanced has been shown to possess no power (24). There is now, by the connexion of the extremities of the series, another junction formed; but it is not direct, being partly made up by the water: and beside this junction, the other circumstance necessary for producing galvanic action is present, namely, the contact of both metals of the arc with the same

portion of fluid. In fig. 1. (see the plate), the strong line in each arc represents zinc, and the weak one copper. When *a* and *e* are connected by the wire (represented by the dotted line), the effective contact is not between the copper *a* and the zinc *e*, for then these metals would not be in contact with the same portion of menstruum; but it is between *a* and *b*, the contact being made up partly by the other metallic arcs, and partly by the fluids. The copper *a* now transfers its affinity for oxygen through the connecting wire to the zinc *e*, and from thence through the arcs and fluids to *b*: the latter transfers back, in return, its affinity for the other component gas to *a*. The next copper *f* transfers through *b* and *a*, through the wire, and round through the other arcs until it arrive at the zinc *c*. In the same manner, *g* transfers to *d*, *h* to *i*, and *k* to *e*. The opposite kind of affinity is transferred similarly, but retrogradely.

27. From this mode of explanation, we see that the connecting wire is the medium of transfer to both kinds of affinity, and to all the arcs conjointly: hence the reason why the accumulation appears to take place here. The accumulation is only apparent; for when the ends are thus connected, the transfer takes place equally through each of the arcs: the zinc legs, throughout, are acted on, not only by the oxygenous affinity transferred by the copper associate of each, but also by the oxygenous affinity of all the rest passing through

their substance. The copper legs, however, are not acted on by the oxygenous affinity passing through them, for the same impulse which caused them primarily to transfer their affinity to the zinc, still continues to operate; so that they cannot act by it, and they merely assist the transfer of affinity to its proper destination. Thus the metals, throughout, are acted on proportionately to the quantity of affinity transferred, and to their surface: therefore the accumulation is only the sum of the different affinities transferred, and it is equally diffused throughout the whole. Had we supposed the active contact to have been the direct junction of the metals, there could evidently be no such circulation; each copper would barely transfer to its associated zinc, and the whole would only act by a power equal in each to that of one. Another proof will appear presently (31).

28. This explanation accounts for a fact ascertained by Sir H. Davy, namely, that equal quantities of gas are discharged in equal times, by each copper plate in a galvanic series, indicating equality of power. But the fact is inexplicable while electricity is maintained to be the cause: a cause which being said to increase in each plate, ought to produce increasing effects.

29. In a single metallic contact, the transfer of affinities is not effective unless the metals be in *direct* contact, or at least through the medium of some other metal or charcoal. When the

transferred affinity arises from many pairs all operating together, the accumulated force is active through the medium even of water. Such is the case in the galvanic series, of which the mode of action has been just attempted to be explained.

30. It has been already shown that the transfer of affinity, from any piece of copper to its corresponding zinc, takes place through all the rest, whatever be the metal, so as to be equal in all; and that this can only cause the oxygenation of the zinc, since whatever originally caused the transfer from the copper must still continue to prevent its combining with oxygen. There is nevertheless a modifying circumstance next to be noticed.

31. That the transfer of affinity, in order to become effective, must be in a state to be satisfied, has been already observed (22. 23. 24.): and therefore each metal of each arc must be immersed in the same portion of fluid, opposite its associate. Now there may be parts, for instance of the copper, which not being under proper circumstances, do not suffer the transfer. Such would be the case if, in the figure before referred to (fig. 1) the arc *c g*, or any other, were entirely of copper. The transfer of affinity for oxygen to the zinc plates takes place from that part *g* immersed in the acid; and the leg *c*, although immersed, is not the proper copper associated with any zinc plate. On the contrary, its

associate *f* is also copper, and from this it even receives affinity. Now through this copper leg *c* is transferred the whole quantity of one kind of affinity acting in the series, and as it does not itself actively transfer, it ought to be passive. That this is so appears from experiment, for in an arrangement which contains one arc entirely of copper, that leg which ought to have been zinc dissolves with great rapidity, while its other leg merely evolves gas without dissolving. This is the fact alluded to (27), which almost proves the opinion that the acting contact of the metals is the long circuit through the arcs and fluids, and not the more direct junction: for had the latter been true, both legs of the copper should dissolve.

The foregoing statements may be strikingly illustrated by arranging a number of glasses in a circle, filling one with muriatic acid, and all the rest with dilute nitric acid. Arcs composed of copper and zinc are to be immersed in the manner of a *couronne des tasses,* but one of the arcs is to be entirely of copper, so that one of its legs may be in the muriatic acid along with the copper leg of a compound arc. The only difference between this and a common arrangement is, that one of the glasses contains a different acid from the rest, and has two unconnected copper legs in it, one of which ought to be zinc. When the circle is complete, it will be found that the copper leg, which ought to be zinc, dissolves in the muriatic acid; although without being affected

by transferred affinity, the copper could not be acted on by that acid in the cold.

32. I ascertained that in all cases of affinity, transferred in consequence of contact, the effects are equally produced when charcoal is substituted for the less oxydable metal. I made many trials of charcoal, in contact with zinc and other metals; and invariably found that the compound piece gave off more gas than the single metal in the same time: but the difference was much more considerable in some cases than in others, and even the same result was not always obtained from the same metal and charcoal, the latter seeming to be uncertain in its effects. This agrees with well known facts: and accounts for the efficiency of charcoal as a member of a galvanic series.

33. To galvanic action, it is not essential that the transfer of affinity should be so perfect as that one only of the metals should dissolve. I have shown (11) that when the acid, to which two metals in contact are exposed, is strong, there is a compensating affinity which makes amends for the affinity lost by the copper, which therefore dissolves in consequence. This however should not influence the result. Although the copper dissolves by this compensating affinity, it is not by its own proper affinity, for this is transferred; and being transferred, it is no matter if the copper be acted on by what may be called a foreign force. In the same manner, if in a galvanic series the acid be concentrated, the copper dis-

solves, but it has transferred to the zinc, and the zinc to it, so that each metal throughout is in an increased state of affinity for each respective class of bodies. According to principles laid down (22), it appears that when the acid is concentrated, the affinities can be satisfied as fast as they are transferred; so that in this case, the galvanic power of the series is at its maximum, and the series is speedily destroyed by the solution of both metals.

34. Zinc acts better in a galvanic series than any other metal. The reason of this seems to be that its natural affinity for oxygen is greater than that of any other that could be conveniently employed. The sum of the natural and acquired affinities is therefore very great. If the zinc be hammered or exposed to pressure, it becomes condensed, hard, therefore less oxydable, and hence less energetic in a galvanic series. This is the reason why milled zinc acts worse in a battery than plates made of cast zinc.

35. Were it possible to employ potassium instead of zinc, there can be little doubt that the effects would be far more powerful. In the same manner, if the other metal were one possessing a strong affinity for hydrogen, it is very probable that the effects would be still stronger. The most energetic of all galvanic arrangements would therefore be one composed of potassium and tellurium.

CHAPTER II.

Application of the foregoing principles to the explanation of galvanic phenomena.

HAVING now given a statement of the general principles relating to transferred affinity, and having applied them to the explanation of what is called galvanic excitement, it remains to try if a more particular application of them will explain those phenomena which have been supposed to result from this excitement. The facts to be explained by these principles are as follow. 1. Metallic arborizations. 2. Phenomena of the chemical agency of galvanic arrangements. 3. Electrical phenomena. 4. Light and heat. 5. Contractions and shock. These shall therefore be the subject of five sections: and metallic arborizations, being the most simple case of the chemical effects of galvanism, and introductory to those more complicated, shall come first under consideration.

SECTION I.

Theory of metallic arborizations.

36. IT had long been known to chemists that when a metal is immersed in the solution of

another, the latter often forsakes its union with the acid and oxygen, and re-appears in the metallic state; while that which caused this re-appearance dissolves in its place. No rational explanation of these phenomena was given until the time of Bergman. This philosopher, and afterwards Lavoisier, considered the precipitation as owing to the greater affinity of the precipitating metal to oxygen. Vauquelin supposed that this affinity must be modified by the affinity of the oxides to the acid.* Berthollet conceived that the preceding causes are by themselves insufficient: he observed that in these precipitations the newly eliminated metal does not separate pure, but in combination with that which caused its separation. Hence he argued that "it is the affinity of one metal for another which determines its disoxydation; that while one part of the metal combines with that which precipitates, another acts by its affinity on the oxygen and on the acid, and that this latter effect would be generally limited to a more or less unequal division either of the oxygen, or of the acid, without the concurrence of the reciprocal action of the two metals." The continuation of the precipitation, he considered as analogous to the increase of a crystal in a saline solution, when

* L'affinité de l'oxigene, plus grand pour tel metal que pour tel autre, n'est pas la seule force qui agisse dans ce cas: il faut aussi faire entre dans le calcul l'affinité de l'acide pour l'un et l'autre oxide metallique: car c'est de ces deux forces que se compose la somme des affinités divellentes ou quiescentes.

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once the nucleus is formed; except that in the case of the metal, the increase is not limited by the solvent power of the water, as happens with the crystals of a salt. Mr. Sylvester was the next who examined this subject. He supposes that the phenomenon is owing to galvanic agency; thus when zinc is presented to nitrate of silver, "the zinc in the first instance reduces the oxide of silver immediately in contact with it: the silver and zinc have now become a galvanic combination," water is decomposed, the oxygen uniting to the zinc, the hydrogen passing to the silver, and causing the reduction of further portions of the oxide. Grotthius, without any knowledge of Sylvester's theory, proposed an explanation which is the same, unless with regard to some hypothetical views relating to the decomposition of water. He also added some new and interesting facts. The outline of this theory is, that the commencement of the precipitation is chemical, as Berthollet and Vauquelin supposed; that water is decomposed; that its elements, by a series of decompositions and recompositions, are attracted and repelled according to their electric states; the latter corresponding with those of the precipitating metal and of the particle newly revived, and that hydrogen is the means of continuing the reduction of the oxide.

Reflecting on the various modes of explaining these singular phenomena, it appeared to me that none of them seemed satisfactory; and that the

explanation depended on the laws of transferred affinity, which I have made attempts to develope. Previously to offering the result of my own experiments, it will be necessary to point out the grounds of objection to the foregoing opinions.

37. Berthollet has shown that the theory of Bergman and Lavoisier is insufficient: his own opinion applies to most of the phenomena; but there are others which seem to militate. I adduce the following.

Into a tall cylindrical glass was poured some solution of nitrate of copper; a little nitric acid was added, and the whole was well mixed. A slender cylinder of polished iron was then suspended in it, so as to remain about half way from the bottom of the glass. Copper instantly attached itself to the iron, and soon formed a kind of casing, which at length separated at one end, but still remained attached to the iron at the other. For a short time it continued to increase, but at length fell to the bottom; and there it speedily dissolved.*

On this experiment we may reason as follows. Iron and copper will each dissolve in dilute nitric acid; which shows that the force by which these metals attract oxygen is, under these circumstances, greater than that by which the latter is

* For the success of this experiment, it is sometimes necessary that the glass should be immersed in cold water, as otherwise the expected results may not be obtained, for reasons that will appear hereafter.

retained by nitrous gas. In the solution of copper, with excess of acid, made use of in the above experiment, there are three combinations of oxygen; one with copper, one with nitrous gas, and one in which both the preceding are united. But the first is retained by a stronger force than the second: as to the last, it may be here neglected. When into this solution a piece of iron is plunged, it will take up oxygen: but it will take it from that compound which offers least resistance, and this, as has been already shown, is the free nitric acid. Hence the iron should dissolve, and no copper should be precipitated: for the copper and oxygen being held by a stronger force, the combination ought not to be subverted: yet the contrary is the fact. Furthermore, it will not be easy, on Berthollet's principles, to account for the increase of the copper casing in an acid which tended so powerfully to dissolve it: nor for its sudden solution, when it fell to the bottom of the vessel, and remained unconnected with the iron. Some other objections will also occur hereafter.

38. The theory of Sylvester and Grotthius being in effect the same, whatever objections apply to one, apply equally to both. Both consider that the commencement of the arborization is chemical, and Grotthius expressly mentions that the reduction of the first particle is effected in the manner supposed by Berthollet. Now if we can ever form a judgment of what simplicity in Nature's operations is, we would suppose that two different causes are not necessary to explain one

effect: and that whatever cause was adequate to commence the reduction, was equally so to continue it. From the experiment above described, we perceive that Berthollet's theory is not universally applicable; and therefore that the commencement of the reduction remains as much a mystery as before. Nor do I consider the theory of the continuance of the operation satisfactory, for reasons which I shall assign.

If copper and silver, in contact, be immersed in nitric acid of a certain strength, nitrous gas is evolved, and without any admixture of hydrogen; nor does any compound of hydrogen appear to be formed. This shows that, in this case, water is not decomposed; that its elements, under these circumstances, are not so easily separated as those of nitrous acid. Now if copper be immersed in a solution of nitrate of silver with excess of acid, and of the abovementioned strength, and if we allow that a particle of silver is already reduced on it, there *may* possibly be a galvanic current established; but we have no grounds for supposing water to be decomposed, at least if we acknowledge the foregoing experiment, which in its circumstances is scarcely different. If water be not decomposed, the admission is manifestly subversive of the theory.

39. Notwithstanding that I cannot coincide in opinion concerning the truth of the internal structure of this theory, yet I think these gentlemen have fully succeeded in the outline, namely, that the phenomena are of that class called gal-

vanic. It appears to me that no opinion can be more improbable than that of supposing the effects to depend on electricity. To understand how a particle of metal, adhering to a mass of another, can assume differently electric states, while both are immersed in a good conducting medium, and how the feeble electricity supposed to be produced can effect changes, to which real electricity of high intensity and great quantity would be inadequate, is past comprehension. I have made many experiments on the effects of electricity on arborizations: the precipitating metal was connected with the conductor of an electric machine, and it was kept strongly electrified, while a glass containing a solution of the metal to be precipitated was raised under it, so as to effect an immersion of the former, while the machine was pouring in torrents of electricity. It is not allowable, in this case, to suppose that there could have been any difference of electric state; yet the arborization went on as rapidly as if electricity were not concerned.

These phenomena seem to admit of a natural and simple explanation by the principle of transferred affinity, which I have endeavoured to develope; and it will be found, that between the catalogue of arborizations and that of transferred affinities there is so striking a coincidence, that much probability is derived by the views which I have brought forward.

40. When a metal is, by filing, reduced to small particles, each particle possesses all the properties

of the original mass; if these filings, by any means, be reduced to much smaller particles, they will be found still to be aggregates perfect in all the properties of the metal. Metals may be reduced to such a state of minute division, as to be impalpable powders. Silver, when obtained by mixing its nitrous solution with sulphate of iron, is a striking example: yet the microscope proves each atom to possess even the splendour of the original metal. When a metal is dissolved in an acid, it is reduced to its component atoms: but we cannot conceive these atoms to exist otherwise than as possessing extension, hardness, density, gravity, nay even splendour, although disguised by minute division, and by union with oxygen and acid. In the solution, each particle is united to one or more particles of oxygen; and without any hypothesis we are compelled to suppose that the particle of metal and that of oxygen are lying in juxtaposition: they must either lie side by side, or both occupy the same space, which is absurd. It is however a common opinion, that when a metal is dissolved, there is a complete destruction of the metallic nature; but such an opinion is not consistent with the present state of knowledge. We can have no other conception of the state of the metal than that it is reduced to its ultimate parts, that each is a perfect atom of metal attracted and joined to a variable number of oxygenous particles, along with those of the base of the acid; the combined atoms be-

ing equally diffused throughout the water of the solution.

41. I have already endeavoured to prove that the contact of dissimilar metals produces a transfer of affinities. Whatever the metals employed be, the change takes place, so that, at least with respect to each other, we may, for convenience sake, divide metals into two orders; the first containing those which transfer their affinity for one class of bodies; and the second containing those which transfer their affinity for the other class of bodies. Let one be selected from each order, suppose iron and copper, and let the copper be dissolved in dilute nitric acid. If the iron be immersed in this solution, it must necessarily come in contact with the diffused particles of copper: and these obeying the general law, will transfer their affinity for oxygen to the iron. The copper has now lost its affinity to the oxygen; the oxygen however retains its affinity for the copper: but this is overcome by the greater power of the iron, which has now received an increase of affinity for oxygen, by contact with the cupreous particles. These particles, being detached from their union with oxygen, are no longer attracted by the acid; the acid therefore is attracted by the affinity of oxide of iron just formed: and the copper, being now unrestrained by any force, is left in the metallic state deposited on the iron, which being thrown into the other state of affinity therefore attracts it. If the solution of nitrate of copper, originally employed, had been

dense, the particles of copper, being close to each other when eliminated, obey their aggregate attraction, and attach themselves to the iron in the form of a hard casing: but if the particles were much separated by dilution, they are, when reduced, out of the reach of the cohesive force; they therefore separate in the state of a fine powder, which, when by accumulation it acquires much weight, falls to the bottom of the vessel.

To express these changes with more simplicity, it may be merely stated that the particles of dissolved copper, when they come in contact with the immersed iron, transfer their affinity for oxygen to it; that the oxygen and acid pass to the iron, while the copper aggregates into masses, and so appears in the metallic state.

42. Having thus attempted to account for the commencement of the precipitation, on principles which I hope are not delusive, it remains to explain the continuation of the process. When the precipitating metal has acquired a coating of the precipitated metal, it might appear that the former, being no longer in contact with the dissolved metal, no further transfer could take place, and that the precipitation should be at an end. The objection is reasonable, but not unanswerable. If a wire of copper and a wire of iron be soldered end to end, so as to form one straight wire composed of these two metals; and if this be immersed in dilute nitric acid, it will be found that the copper gives out gas in *every* part, and that no part of the copper dissolves. Hence it appears

that, by whatever cause the loss of affinity is produced, the transfer takes place equally well at that portion of the copper furthermost from the junction with the iron, through those parts that are nearer. This once duly considered, the objection is removed: the particles of metal not yet precipitated transfer their affinity to the precipitant through the medium of those already deposited: and thus the change goes on to the end.

43. If this explanation be an approximation to the truth, and if the first portions of copper precipitated only convey the transfer of the subsequent portions, I conceived that the same effects ought to be produced, if metallic copper were presented to the solution, provided this copper were connected with iron, to which the affinity could be transferred: and that copper, so connected, would act the part of the first reduced portions of copper in transferring to the iron the affinity of those not yet reduced. The following experiment seemed to coincide with the supposition.

A cylindrical glass vessel was half filled with a strong solution of nitrate of copper, and the other half with water, poured on so gently as to float. A bar of copper and a bar of iron were joined end to end, by a copper rivet, so as to form a compound metallic bar of such length as to reach from the surface of the fluid to the bottom. The copper end and juncture stood a little above the nitrate: the iron part was therefore in the water, and did not touch the solution beneath. In a very short time, flocks of peroxide of iron appeared in

the water, and small points of reduced copper began to attach themselves to the copper in the nitrate, while the iron was dissolved near its junction with the other metal.

These results were far more strikingly obtained when the experiment was made with nitrate of silver, water, and a compound bar of silver and copper. The water very soon assumed a bright purple colour, owing to the diffusion of oxide of copper, and at length beautiful filaments and studs of bright silver attached themselves to the silver end of the bar.

44. It appeared probable that any metal, which would transfer affinity to the precipitant metal, might be made the medium of transfer, between the dissolved metal and the precipitant, even though by itself it were not effective. The last mentioned experiment was repeated, substituting nitrate of copper for that of silver, and employing a bar half silver and half iron, the former metal being immersed in the solution of copper. In a short time, peroxide of iron floated in the water, and the silver was coated with copper. Now it is plain that the silver acted only as a medium of transfer between the dissolved copper and the iron, for by itself it does not possess a power of precipitating copper: and as a further proof of this, when the iron is removed, the copper oxydizes, and quickly redissolves. These facts afford an explanation of Dr. Wollaston's experiment, in which silver and iron, in contact, became coated with copper, in a solution of that

metal. The dissolved copper transferred its affinity both directly to the iron, and indirectly through the mediation of the silver, and hence copper was reduced on both. Corresponding with this explanation, we find that if a piece of copper wire be joined to one of silver, and the latter to a piece of iron wire, the iron alone will dissolve, when they are immersed in dilute nitric acid; for it receives affinity from the other two.

The same explanation applies to the curious experiment of Grotthius, in which a glass cylinder was half filled with solution of nitrate of copper, and the other half with nitrate of silver. A piece of zinc being suspended in the solution of silver, an argentine tree was formed, the branches of which extending downwards, at length reached the cupreous solution, and there became tipt with copper. The dissolved silver transferred its affinity to the zinc, the reduced silver then became a medium through which the copper transferred its affinity, and this also was reduced: so that the zinc received the affinity of the other two metals.

This explanation equally applies to Sylvester's experiment, in which a glass plane was half coated with nitrate of silver, the other half with dilute muriatic acid: a platina wire was laid on the nitrate, and one of zinc on the acid. When both wires were connected at their further ends, the platina soon gave off ramifications of reduced silver: because the affinity of the latter for oxygen

was transferred to the zinc, through the intermediate platinum.

45. These changes are materially assisted by the affinity for hydrogen, for metals, and for all other bodies of the second class, which the metal transferring its affinity for oxygen acquires by its contact with the other. If a piece of silver and a piece of copper, in contact, be immersed in dilute nitric acid, the silver remains unaltered until towards the end of the process; it then blackens and acquires a coating of copper. The reason of this is as follows. The first dissolved particles of copper are retained more strongly by the acid than they are attracted by the silver; but as the solution advances towards saturation, the particles of copper are less forcibly attracted by the acid; and the affinity for the second order of bodies, which the silver acquires by contact with the undissolved mass of copper, exerts an attraction stronger than that of the acid: hence, the silver acquires a coating of copper. These principles account for the fact mentioned by Grotthius, namely, that in his experiment already mentioned, a piece of copper effected the reduction first of the silver, and afterwards of the underneath dissolved copper; but the effect was much better produced by zinc.

From the preceding experiments and observations, it appears that the commencement and continuation of the process of arborization are

produced by the same cause; and that therefore this explanation is not liable to the objections which I have already applied to former theories. In the experiment with the compound metallic bar (44), the less oxydable metal transferred affinity to the more oxydable: and the particles of dissolved metal also transferred their affinity through the medium of that to which they attach themselves. The activity of the particles reducing is plainly discovered in the precipitant metal which oxydized even in water, although the bar of two metals placed simply in water would not be affected.

46. In metallic arborizations, I have often found that when the precipitant became well coated with the dissolved metal, the increase of the arborization was greatly retarded if not suspended. This agrees with the preceding views. Although the dissolved particles transfer their affinity for oxygen to the precipitant, yet the transfer becomes inefficacious, as the affinity cannot be satisfied (22. 23. 24).

47. Such are the principles by which I have attempted to explain the phenomena of metallic arborizations: and I conceive the explanation to be free from the difficulties which attend the electro-galvanic hypothesis. If the views which I have brought forward be correct, it ought to happen that the order of precipitation will correspond with the order of solution and transfer: and

that this is so appears from the following table. In the first column, are placed those metals on which experiments were made to determine the order of transfer (7. 8); the first mentioned metal in each pair being that which transferred its affinity, the second being that which received it, and dissolved with increased force. In the second column are placed the same pairs of metals, showing which is precipitated and which is the precipitant: and it will be found, invariably, that the precipitants are those which receive affinity from the precipitated metal when they are in contact.

TABLE

	OF TRANSFERRED AFFINITIES.		OF PRECIPITATIONS.
1.	Copper with zinc.	1.	Copper by Zinc.
2.	——— Iron.	2.	——— Iron.
3.	——— Lead.	3.	——— Lead.
4.	——— Tin.	4.	——— Tin.
5.	——— Bismuth.	5.	——— Bismuth.
6.	Silver with Copper.	6.	Silver by Copper.
7.	——— Zinc.	7.	——— Zinc.
8.	——— Lead.	8.	——— Lead.
9.	——— Tin.	9.	——— Tin.
10.	Mercury with Copper.	10.	Mercury by Copper.
11.	——— Iron.	11.	——— Iron.
12.	Lead with Tin.	12.	Lead by Tin.
13.	Tin with Zinc.	13.	Tin by Zinc.
14.	Gold with Zinc.	14.	Gold by Zinc.
15.	——— Copper.	15.	——— Copper.
16.	——— Iron.	16.	——— Iron.
17.	Platinum with Zinc.	17.	Platinum by Zinc.
18.	——— Copper.	18.	——— Copper.
19.	Palladium with Zinc.	19.	Palladium by Zinc.
20.	Cobalt with Zinc.	20.	Unknown.
21.	Nickel with Zinc.	21.	Unknown.
22.	Arsenic with Zinc.	22.	Arsenic by Zinc.

The only anomaly that occurs here is in the case of copper with bismuth: for copper is said to precipitate bismuth, and I find that bismuth, immersed in acidulous nitrate of copper, gets a partial coating of the latter metal. I have already remarked that these two metals, when immersed in contact in dilute nitric acid, both dissolve. Hence we can only conclude that the transfer of affinity in whatever order it may be, is so feeble as to be liable to variation from inconsiderable causes. Berthollet cites an instance from Vauquelin in which lead was stated to have precipitated zinc. On referring to Vauquelin's paper I do not find such a statement: and I have myself made the experiment without this result: from all which I conclude that the discordance may proceed from some typographical error. The correspondence of all the other instances seems a corroboration of the explanation which I have offered. As to the precipitation of platina by zinc and copper, the transfer seems to take place at least in some degree: a dark coloured precipitate appears which does not possess the least lustre; the dissolved oxide perhaps loses but a portion of its oxygen.

48. I have noticed (11) what I called the compensating affinity of acids. The power of this is such that the loss of affinity in one of two metals in contact may be compensated by bringing the combined effects of many particles of oxygen to

act within a small compass. Now it appears but fair to conclude that the same compensation ought to take place in the precipitation of metals, which is nothing more than the converse process. This is accordingly the case: for if copper be dissolved in an excess of tolerably concentrated nitric acid, the copper does not re-appear when iron is immersed. The reason is obvious: although the particles of dissolved copper transfer their affinity to the iron, yet the affinity of the great number of oxygenous particles compensates the loss, and the copper remains dissolved. But this fact does not militate with that stated in the commencement of this section as an objection to Berthollet's theory: for in the latter case the acid was not sufficiently concentrated to compensate the affinity transferred.

49. There are cases of metallic precipitations to which the foregoing principles do not apply with equal ease. There seems no reason to suppose that the transfer of affinities is an exclusive property of solids: we may with more probability conclude that heterogeneous fluids when brought into contact will suffer some analogous change, and affect a metal as two metals would a fluid. This supposition flows from the following fact. If a small cylindrical glass be half filled with nitrate of silver and the other half with water; and if a bar of silver be gently let down, so as not to cause the mixture of the two fluids; it will

be seen, in some time, that the part of the bar in water will oxydize, while that part in the nitrate will become studded with reduced silver. Thus a change of affinities seems really to have taken place: the silver under common circumstances will not oxydize in water: and we see that the affinity of the dissolved metal is destroyed since it becomes reduced. The same happens with a number of other metallic solutions; and whoever consults the curious experiments of Bucholz, will probably agree in this supposition that the phenomena may depend on the transfer of affinities between the fluids. I have repeated many of Bucholz's experiments, but observed nothing which was not noticed in the accurate and descriptive details of that chemist.

It is no doubt by the same principle that we are to account for the galvanic series which have been formed of two different fluids and one metal or charcoal.

50. According to my view of the subject, metallic arborization is no more than the inverse of transferred affinity. In one case, when two metals in contact are presented to an acid, the less oxydable transfers its affinity for oxygen to the other and remains solid: in the other case, when the less oxydable metal is already dissolved, it transfers its affinity for oxygen to the precipitant, and becomes solid: and in both cases, the more oxydable metal dissolves with increased

force. So that precipitants are always those metals to which affinity is transferred.

SECTION II.

Of the general chemical effects of galvanic arrangements.

51. ALL the preceding statements concur in rendering it probable that, in every galvanic arrangement, one series of metals is thrown into a state of increased affinity for one class of bodies, and the other series into a state of increased affinity for the other class: the whole force being the sum of the different affinities transferred. Taking this view of the subject, it is not wonderful that compound bodies having their elements comparatively feebly united, should, when presented to the galvanic arrangement, obey the attractions of the more energetic affinity.

Oxygen and hydrogen seem to be the leading bodies of both classes, according to the habitudes of which, all other bodies comport themselves. From the experiments of Davy, Berzelius, and Hisinger, we learn that oxygen, and bodies in which oxygen predominates, and oxymuriatic acid, pass to the zinc side of the series; and it is shown that this side, and bodies connected with

it, are in a state of affinity for oxygen, and for the other substances of that class. Inflammables, metals and their oxides, alkalies, and earths, pass to the copper side, which, as we have seen, is in a state of increased affinity for these bodies. Here then the general facts agree perfectly with the principles which I have offered to explain them.

It is evident, when the elements of bodies are separated, that they may be transferred through substances to which they have a strong affinity: for the natural force of affinity is weak when compared to the accumulated affinity of the decomposing power: and as shall be shown, the elements during decomposition may, at least in some cases, also suffer a transfer of affinity analogous to that of the metals in the series. The facts of this kind ascertained by Davy would, in this manner, admit of an easy explanation: and it would seem not to labour under the disadvantage of that philosopher's statement, namely, that as the transferred elements are supposed to be in a state of electrical attraction as intense as that which effects the decomposition, they should rather combine more forcibly with the bodies through which they are transferred, than pass to their destination.

52. On account of convenience, these decompositions are generally effected at what is improperly called the extremity of the galvanic series: but the same might be accomplished in any part

of the series, as each pair of metals is in an equally energetic state of affinity. For instance in the case of water, the decomposition takes place between all the plates, as was first conceived by Nicholson. If the fluid, placed in the glasses of the series, be nitrate of copper, copper is reduced in each, as well as it would be at the extremities. As to the common expression *extremity*, it implies an error: the series is really a circle; it has neither beginning nor end, until interrupted, and then it is no longer a galvanic series, being inactive.

53. The wires, between which the series is interrupted, are conceived to be in a state of increased affinity for the two orders of bodies. In many cases the wires and bodies attracted to them obey this affinity and combine. Thus any oxydable metal, if placed on the zinc side, will combine with the oxygen which passes to it from any compound: and arsenic or tellurium on the copper side, will combine with hydrogen. Sir H. Davy brings it as a fact strongly in favour of his hypothesis, that a copper wire on the copper side of the series will not dissolve in nitrous acid; because, as he supposes, they repel each other by being in similar states of electricity. But this in my mind is far from offering any support to his opinion; for a wire of copper in contact with a wire of zinc also refuses to dissolve, yet the electricity, if any be produced, is not sensible to the

most delicate electrometers: while, on the other hand, a copper wire made to discharge the utmost negative power of a large electrical machine dissolves with facility. Now on the principle of transferred affinity we discover a simple and sufficient reason not liable to this objection, and this is the transfer which takes place from the copper to the zinc of the series throughout, which must also affect the copper wire.

54. There is also another power acting which assists the former in preventing the solution of the copper wire. We know that if the conducting wire from the negative side, be immersed in solution of copper, the dissolved metal would resume the metallic form, because the conducting wire possesses an artificial affinity for it superior to the attraction of the menstruum. Since the copper cannot remain dissolved when presented to the negative side, the same power which causes this should also prevent already metallic copper from dissolving in an acid. Thus, there are two forces counteracting the natural tendency of the copper wire to dissolve in nitrous acid. Yet both of them may be overcome by the compensating affinity of the nitrous acid, if it be even but a little concentrated.

55. If a zinc wire be placed on the negative side, it does not dissolve in very weak nitrous acid, for it is only acted upon by the latter of the

forces abovementioned (54), as zinc cannot transfer its affinity for oxygen to the copper side with which it is connected. Since its natural tendency to dissolve in the acid is counteracted by one power only, it is accordingly found to resist the compensating affinity of nitrous acid with much less force than copper; for an acid so far diluted as to be incapable of acting on the copper wire on the negative side, will readily dissolve a zinc wire similarly circumstanced. All these facts are inexplicable on the electrical hypothesis.

56. If a copper wire, connected with the copper end of the series, be immersed in water, the hydrogen is evolved, and does not combine with the metal. The reason of this has been already assigned (13. 15). Whatever may be the force of affinity between the copper and hydrogen, it is not great enough to oppose the obstacles which the cohesion of the metal, and the elastic tendency of the gas offer. The particles of hydrogen therefore join each other round the copper; a perceptible bubble is formed, which rises by its specific lightness, in opposition to the force of affinity: for the natural affinity of copper to hydrogen being feeble, it gives very little aid to that which is acquired. When the affinity is naturally very energetic, we find that the hydrogen does combine; this is the case with tellurium and arsenic, when on the negative side of the arrangement.

57. When a platina wire, connected with the zinc end of the series, is made to decompose water, the oxygen does not combine with the metal, but is evolved. This may be explained in the same manner: the natural affinity, feeble indeed, of the platina for oxygen, added to its increased affinity, is not sufficient to overcome the obstacles to union. If the natural affinity be somewhat greater, as when a silver wire is used, the natural and acquired affinity conjointly will enable the silver to unite with oxygen: although, without the acquired power, the silver could not do so. The power of natural affinity is strongly shown in tellurium. That metal if on the zinc side does not oxydize, its splendour is not even tarnished; but because it has a strong natural affinity for hydrogen, its cohesion gives way, when on the copper side, and they combine: although without the acquired affinity, even this could not have been effected.

58. As to the manner in which decompositions are produced, it is of no other importance than that it renders less difficult the explanation of certain phenomena not hitherto understood. The simplest case, and that which will best illustrate the rest, is the decomposition of a metallic salt.

For the sake of simplicity, suppose the galvanic series to be composed of zinc and silver, terminating at the zinc side with a zinc wire, and at the silver side with a silver wire: and let nitrate of

silver be placed in the circuit for decomposition. All the silver plates transfer their affinity for oxygen to the zinc, and all the zinc plates their affinity for the other class of bodies to the silver. The silver wire also contributes to the transfer, and the row of dissolved particles of silver between that and the zinc wire should do the same. Thus the dissolved metallic particles have, to a certain extent, lost their affinity for oxygen: the combination is therefore subverted, or very much weakened; and the elements are at liberty to obey their respective attractions, and to collect round the wires which are possessed of these energetic affinities. As to the acid, it has but little affinity for the metal, when the oxygen is withdrawn; it therefore gives way to the new arrangement by similar means. Thus two forces tend to the reduction of the dissolved metal.

But if the metallic solution submitted to the action of the zinc and silver be solution of zinc, the case is somewhat different. In this case, the dissolved particles of zinc cannot transfer affinity to the silver of the series as in the former case. The reduction of the metal is therefore entirely produced by the strong artificial affinity, induced on the silver wire of the silver side, for all bodies of the second class. The dissolved zinc is attracted to the silver side, by a force superior to that of the acid with which it is united; and the acid is attracted equally from the zinc with which it was united, by the zinc side of the apparatus. In the

former case, wherein nitrate of silver was submitted to decomposition, the silver was reduced by two forces; in the present instance, the zinc is reduced by one force only; and accordingly the reduction is effected with more difficulty, as will be found by making comparative trials on the two salts. In the same manner, potassium requires a very extensive series, and intense power.

59. It is now necessary to explain the various cases of the decomposition of water, according to the principles which I have endeavoured to support. The simplest case is that wherein the extreme wires of a galvanic series are immersed at a little distance from each other in water. It is evident that the space between the wires is occupied by a thread (as it may be called) of water. This thread is composed of particles of oxygen and hydrogen, arranged as in fig. 2. (see plate). When the circuit is completed, the energetic transferred affinities begin to operate, and each gas gathers round its proper wire; the row of particles of oxygen, as it were, sliding along the row of particles of hydrogen, and the thread of water being continually maintained by the flowing in of new supplies (fig. 3). According to this view, the attraction of the particles of oxygen to those of hydrogen still continues to operate, but it is overcome by the stronger attractions of the wires. When the zinc wire has attracted a particle of oxygen, a particle of hydrogen would be

set free, but for the sliding of the particles over each other. On this account, the particle of hydrogen immediately comes in contact with a particle of oxygen, which in the same manner was set free by losing its associate hydrogen; the new combination therefore takes place, but it is only of momentary duration, as the same change takes place with regard to a new pair of particles. The circumstances of this decomposition may be mechanically represented as follows. If two strings of beads, of equal length, lie in apposition, they will represent the thread of water before decomposition. If each be drawn at the opposite ends, so as to slide over each other, they will represent the water during decomposition. In this view, it is not supposed that there is a series of decompositions and recompositions: the same instant that a particle of oxygen passes off from its associate hydrogen, it comes in contact with another, without having been left detached for a moment, therefore the composition is maintained uninterruptedly, except that the particles change their associations. Nor is it supposed that there is a repulsion of elements from either pole, nor reproduction in the middle, from their union: hence I conceive the explanation to be free from the objections which lie to that of Sir H. Davy. It appears also that it is unnecessary to suppose the more intricate attractions and repulsions of the two gases, brought forward by Grotthius to explain these phenomena.

It might appear that affinity is here conceived to operate at a sensible distance from the wires: but this is not so. Each row of particles is attracted to its proper wire, the more distant through those that are nearer: for transferred affinity acts through continuous matter. Thus if a wire of zinc and a wire of copper soldered end to end, be immersed in dilute sulphuric acid, the copper end farthest from the zinc discharges hydrogen, and the zinc farthest from the copper dissolves with increased force,—changes which could not happen, unless affinity had been transferred from distant parts through these that were nearer.

60. There are cases of this decomposition which had been long considered inexplicable, and which, notwithstanding the attempts of ingenious persons, are still not less involved in obscurity. One of them is the separate evolution of the gases, at the wires terminating the galvanic series. That indefatigable and able investigator, Sir H. Davy, at an early period of his inquiries, found, when each of the wires was immersed in a separate glass of water, and the glasses were connected by immersing a finger in each, or by muscular fibre, or a wet thread, that the evolution of the gas was still separate at each wire. In a second trial, the water in the glasses was connected with the ends of the series by muscular fibre, and the glasses were connected together by a wire. Here also the wire gave out the gases

separately, but in an inverse order; the end next the zinc giving out hydrogen, the other oxygen. In a third experiment, the water being connected with the series as before, he held one end of a wire in his hand, and immersed the other end in one of the glasses of water: and then connected both glasses, by plunging a finger of the other hand into the other glass: the wire was oxydated, but no hydrogen appeared. When the order was reversed, hydrogen was liberated without oxygen. In his fourth experiment, the glasses were connected with each other, and with the series, by muscular fibre. In this case, no gas was evolved. From these experiments, he drew the singular conclusion that the gases, or one of them, had passed through the metal, from one glass to the other; although, as he expresses himself, "such a supposition is incommensurable with all known facts."

On the view which I have suggested, it appears to me that there is no material discrepance between the explanation and facts: I shall therefore endeavour to reconcile Sir H. Davy's results to the preceding principles.

In the first experiment, if instead of muscular fibre, a syphon filled with water had been used to connect the two glasses, both might be considered as one vessel of water. If fibres of amianthus, or threads which retain water by capillary attraction, were used, the effect would be the same: or even if the connexion were made by wet pa-

per. The case, as Grotthius observes, was the same with moist muscular fibre. The fingers immersed in each glass produce the effect; for the skin is furnished with numerous exhalant vessels, which continually stream out perspiration, in some persons perceptibly to the naked eye, but in all cases to the microscope. This fluid is connected with the mass of blood, and one of the chief constituents of blood is water. So that the two glasses were in fact connected by water, and we may therefore consider the two portions as but one. As to the other parts of the fingers, the skin, flesh, &c. they are in themselves insufficient. I have often received severe burns by touching the conducting wires of 1000 pairs of four inch plates, and always observed that the parts thus seared were no longer capable of communicating a shock while dry, although the surrounding parts transmitted the shock without being moistened, such was the intensity of the battery. The cause of this was, no doubt, the separation of the epidermis from the stock of animal fluids. Such experiments as these of Sir H. Davy do not succeed immediately, if the fingers have been previously perfectly dry: but as the skin macerates in the water, the connexion between the animal fluids and the water of the glasses is established, and the effect begins to be produced. Grotthius supposed that the fluids in the finger received electrical polarities: that when one particle of oxygen is attracted to the

positive wire, its hydrogen repels the next particle of hydrogen, and unites to its oxygen; that the repelled hydrogen transmits its motion to the next particle of hydrogen, where the same changes take place. But this seems liable to the objections which apply to Davy's hypothesis of electro-chemical affinity. Perhaps the following is a simpler view of the subject.

In fig. 4. (see the plate) the continuity of the two wires, of the water, and of the fingers, is drawn in a right line for greater simplicity: the water of the two glasses being represented by *a* and *b*, and the fluids of the fingers by *c*. Things are now in a state of rest. When the decomposition commences, (fig. 5.) the strong transferred affinities, operating on the wires, cause the gases to collect round them, as in the case wherein nothing but water was placed in the circuit: the circumstances are the same, and therefore there is no more difficulty in this than in the other case. The animal fluids remain exactly the same in their nature; for each component gas of their water is always associated with the other gas, although the associated particles may not be identical throughout. Even this small change is very limited, for when the glasses are connected by the fingers, the quantity of gases produced at the wire is exceedingly small.

In the second experiment of Sir H. Davy, the glasses of water were connected to the ends of the pile, by moist muscular fibre. He states that

he was much surprised at seeing the wire, which connected the two glasses, calcining at the end immersed in the water next the silver side, and at the zinc side giving out hydrogen. The foregoing principles apply also to this fact. We shall suppose a *couronne des tasses* to be used instead of a pile, the heavy lines in fig. 6. representing zinc, as before. The vessels *a* and *b*, being connected by the fluids of the muscular fibre *c*, may be considered as but one vessel of water: and therefore the end *d* of the silver wire may be said to be immersed in the vessel *a*. If it were so, and if it were connected with the opposite end of the series, it would give out hydrogen. It is in effect exactly under such circumstances; so that the evolution of hydrogen at *d* is what should be expected, and the oxygen, there supposed to be liberated, does not pass through the wire to *e*, but is found oxydizing the zinc plate *f*, where it also would be liberated, were *f* of platina. On the other hand, *g* and *h* may be called one vessel, because connected by muscular fibre *k*, and *e* is virtually immersed in *h*, where it should oxydize when connected with the opposite end of the series, as it really is. The hydrogen that belongs to the oxydizing metal at *e* does not pass through the wire to *d*, but is found liberated at the silver plate *l*. All the phenomena seem so far conformable.

We have now a guide to the third experiment, in which he held one end of a silver wire in one

hand, and immersed the other end in a glass of water, the latter being connected to another glass by immersing in it a finger of his other hand, (fig. 7.). According to what has been just explained, the zinc plate *b* oxydizes; the wire in the hand at *c*, as it would if placed appositely in the vessel *d*, gives out hydrogen: and the wire *c* may be said to be in *d*, since it is in a vessel connected to it by the water in the muscular fibre *e*: the rest of the chain is made up of the animal fluids in the hands *c g*, of the water in *f*, and of the other muscular fibre *h* terminating in the water of *k*. The moisture in or on the surface of the finger, holding the wire at *c*, was decomposed; for that end of the wire, if immersed in the water of *k*, would oxydize, which would exactly correspond with the evolution of hydrogen at *a*. The small quantity of moisture in or on the finger will be no objection, when it is recollected how extremely feeble all the effects are stated by Sir H. Davy to have been. In experiments of this kind, the part of the wire held in the hand tarnishes, which proves that the moisture of the finger is decomposed. When the wire was held in the hand *g*, all the effects were reversed, and the wire in the water oxydized.

In the fourth experiment, wherein the glasses were connected to each other, and to the ends of the series, no gas was given out in either of the two glasses. In this case, the last zinc plate of one end may be said to have been in the same

vessel as the last silver plate at the other, the former oxydizing, and the hydrogen appearing at the latter ; precisely as if the series were arranged in a circle, without any interruption.

These four experiments have been explained on the supposition that a *couronne des tasses* was employed : it is evident that the case is the same were the muscular fibre applied to the ends of a pile, but the explanation would be less simple.

61. There are yet some cases of the decomposition of water, which present phenomena, as I think, totally irreconcileable with the electrical hypothesis which has been framed to explain the general facts. They indeed seem to have been in a manner overlooked.

The instruments called galvanic batteries, as they are at present made, consist of plates of zinc and copper screwed to bars of wood, and immersed in porcelain troughs divided into cells ; each cell containing a zinc and a copper plate, communicating by the fluid alone. The first cell contains but one plate, which is, suppose, zinc : the last also contains one, but it is of copper. When this series is to be employed, a single copper plate, from which proceeds a wire, is loosely put into the cell containing the single zinc plate : and a zinc plate, also terminating in a wire, is placed in the cell which contains the single copper plate. When the other ends of these wires are immersed in a glass of water, that proceed-

ing from the loose copper plate oxydizes, while that from the loose zinc plate discharges hydrogen. This, in whatever manner it may be viewed, is surrounded with difficulty. Although it would appear from the experiment that the end proceeding from the loose copper plate is what is called the oxygenizing pole, and that from the loose zinc the hydrogenizing; yet this is contrary to all opinions. We must, therefore, from this, conclude that the two unconnected plates and wires are generally supposed to answer merely the end of conveying the electric state from the connected series, the loose copper plate conducting positive electricity from the zinc, and the loose zinc plate conducting negative electricity from the copper. How then does it happen that the positive plate *a* (fig. 8.), which terminates in the oxygenizing wire, gives out hydrogen, and that the negative plate *b* is oxygenizing præternaturally? If, on the other hand, it be allowed that the plates *a* and *b* do not merely conduct, but assume the state of electricity opposite to that of their associates, and so become active members of the series, then a total change of the electric states, assigned to bodies by Sir H. Davy, should take place: oxygen must be positive, because attracted to a negatively electrified surface *c*, and hydrogen must be negative, because attracted to *d*, which is positive. The electrometer indicates that the wire *a c* is positive; the hypothesis maintains that hydrogen is also positive: how then is

this gas attracted to *a* where it is evolved? The electrometer proves *b d* to be negative; so is oxygen: yet it is attracted to *b*. Should any one conceive that *c* is made positive by the proximity of the negative wire *d*, the difficulty would not be removed, but increased; for beside many other objections, it is just as unaccountable why *d* should be negative.

The electrical hypothesis being thus encumbered with difficulties, and the phenomena themselves having never been adverted to, nor attempted to be explained, I shall make trial to reduce these contending phenomena under the principles already stated.

We have seen (31) that in a *couronne des tasses*, when one of the arcs was entirely of copper, that leg which should have been zinc performs the office of this metal, and dissolves with increased force. Let us suppose this entirely copper arc to be *a b* (fig. 9.) at the end of the series. In the vessel *d*, the association is perfect, *c* being zinc and *b* copper. The next in order *a* ought to be zinc, but being copper, it dissolves rapidly, as the zinc would have done, for reasons above referred to (31), as soon as the circle is completed. Now if instead of a plate, we consider *a b* a copper wire, it will be precisely under the circumstances of *a c* in the former figure. In the vessel *g*, the metals are properly associated, supposing *e f* to be a zinc wire: but its end *f* ought to be copper, in order to correspond with *a*, which acts

the part of zinc. Now although *f* be zinc, it virtually perfects the association in *h*, and for reasons already assigned (*54. 55.*), it does not oxydize, but discharges hydrogen, and thus performs the office of copper. This gives us the reason why, in the former figure, *d* gave out hydrogen.

There are still more complicated cases of the decomposition of water, but they are reducible to the simple one above stated. The vessels *a a a* (fig. 10.) containing water, and the wires *b b b b* are passive members of the circle. The wires may be of platina; at one end of each, oxygen will be discharged, and at the other hydrogen: but they owe their effects entirely to the action of the regular metallic combinations. The presence of these passive members considerably impairs the energy of the arrangement.

62. The electrometer proves that all the plates at one end of a galvanic series, whether zinc or copper, are positive, without alternation of negative electricity: and at the other end, that they are all negative, without alternation of the positive state. It is therefore quite inadmissible to maintain that the decomposition of the menstruum contained in the cells of the trough at either end, can be the effect of opposite electricities induced on the plates immersed in these cells. There are also numberless other objections to the electrical hypothesis which do not seem incidental to the view here suggested.

After all that has been said, it will be unneces-

sary to state explicitly the manner in which other compounds are decomposed: the elements are transferred according to their respective attractions.

SECTION III.

Of the electrical phenomena manifested by galvanic arrangements.

THE system of the universe is sustained by forces which by their action and opposition balance each other, and preserve that moderate state which permits the changes of things to take place without violence or confusion, and ensures the continuance of existence. This may be observed in beholding creation under every point of view. The power of gravitation continually impels the earth towards a centre; the unlimited operation of this impulse must speedily cause the destruction of our planet, but for the opposing force which keeps her in her orbit. The energy of cohesion is essential to the solidity of matter; but were it unrestrained, the elements of matter must obtain the state of contact, an adamantine constraint would pervade nature, every thing would be still and immutable, life would be extinguished, and matter without design. Cohesion

is therefore opposed by a power which preserves the atoms at the distance necessary to the changes and order of existence. But even this congenial power which softens the rigidity of unyielding matter, when uncontrolled is not less destructive in its effects: a self repulsive force, by preventing its accumulation, moderates its action, and gives it a tendency to equable diffusion.

Amidst the general system of action and counteraction, it is not to be supposed that the most energetic of all the acting forces of matter should have no opposing powers. The illustrious Researcher into the laws of affinity has shown that this attraction is opposed and modified by a number of circumstances, which he has investigated and elucidated with acknowledged ability. It is not here necessary to bring into view the opinions of that philosopher: the object is to consider a few of those circumstances which affect affinity, and to point out between them certain relations which immediately bear upon the subject under examination.

The most obvious force, opposed to the attraction of heterogeneous particles, is the attraction of those particles to each other. Those substances which have a strong affinity for each other, and which easily unite when their particles have freedom of motion, refuse to do so when the power of cohesion is superior to that of affinity: and such is the opposition of these forces that one is always impaired in proportion as the other

exerts energetic action. The next antagonist to chemical attraction is caloric. Bodies mutually attractive of each other are often prevented from combining, by raising their temperature: and even when the combination has already taken place, an increase of temperature will subvert it. In this manner, heat effects the decomposition of the most energetically combined compounds, and certainly may so far be considered an antagonist to affinity. Light must be considered under the same point of view, as it is generally admitted to be capable of separating certain elements which are not very strongly combined. But although these three powers, cohesion, caloric, and light, are to be considered antagonists to affinity, yet this is true under certain circumstances only: there are others under which they rather promote combination. The influence of cohesion has been long acknowledged: but until the researches of Berthollet, it was conceived to be merely a quality of bodies actually solid, which was destroyed by fluidity. It is however a characteristic principle of this philosopher's views, that the attraction of cohesion exists after the destruction of the aggregate, that it is only suspended during fluidity, that it continually tends to exercise its power, when it can do so with effect, and by this effort that it often determines a combination, which otherwise might not have taken place. In like manner, caloric is known to produce combination: and this it seems to do, not only by

diminishing aggregation, but by exalting the natural chemical attractions of bodies. Light is also in some cases efficacious in producing combination: the manner is not known, but the effect is certain.

It appears, therefore, that cohesion, caloric, and light, exert different powers as the circumstances vary: in some cases they oppose, in others they promote affinity. There is one other force which in both properties seems to agree with the preceding: a force which has never been traced as to its uses in the economy of nature, but which, from its astonishing attributes, cannot be supposed inactive in the constitution of matter. Experiment indeed proves that this power called electricity greatly influences affinity: it is known to separate combined elements from each other: and after the separation, the affinity of the same elements may be called into action by electricity, so as to effect a new combination. Thus a striking analogy obtains between these four forces, cohesion, caloric, light, and electricity: if one be considered an antagonist to affinity, so must the rest, and therefore electricity, which is the more immediate subject of the present inquiry.

There are numerous facts which prove the connexion of electrical and chemical phenomena. When water, ether, or other fluids assume the vaporific form, or when from this state they become fluid, electricity is evolved so abundantly that by peculiar management sparks may be ob-

tained. During the transition of sulphur from the fluid to the solid form, electricity appears: and if the cooling be performed on a plate of copper or zinc, the attraction is so strong as to affect large pith balls. The cooling of chocolate, and the solidification of wax in forming candles, are also productive of electricity. The tourmalin is well known, when heated, to produce not only attractions and repulsions, but even flashes of electric light. But the most applicable facts are the following. Lavoisier and La Place found that by the action of dilute sulphuric acid on iron, electricity is evolved; and Volta even obtained a spark, by means of his condenser. I found that by placing a bit of burning charcoal on a gold leaf electrometer, and gently blowing it with a bellows, the gold leaves diverged nearly half an inch.

From these instances, it appears that the exertion of chemical affinity disturbs the repose of electricity; and these and the considerations already adduced render it probable that affinity and electricity are really antagonist forces. In laying down this position, there seems to be but little of hypothesis; and if any, it lies perhaps rather in the words than in the thing: for it is merely another expression of the fact, that the exertion of one power calls the other into action; and this is a true inference from observation.

This being admitted, it is not surprising that in the galvanic series, wherein the most energetic

kind of affinity is going forward, there should be a constant production of electricity. In the pile or other arrangement, there is a continual oxydation of a conducting substance; and that this is a cause sufficient to excite electricity is proved by the experiments of Lavoisier and La Place with iron and sulphuric acid. The electricity produced by burning charcoal is a similar and less equivocal case; charcoal is a conductor, and during its combustion it is oxydated. This shows that mere oxydation might have produced electricity in the case of the iron, and that the effect need not have been dependent on evaporation, as has been gratuitously supposed. It is possible to apply this position more particularly to the two states of the pile; and even to assign a reason for the electricity produced, in cases where sensible chemical action does not take place, as in Volta's plates, a case which has been considered subversive of the opinion that the electricity was dependent on chemical action.

The plates of Volta may be considered the electrical element of the pile; they therefore require to be first explained. We have seen, by numerous examples, that when two dissimilar metals are brought into contact, there is a mutual transfer of affinity of a different kind: the oxydable metal acquires an increase of one kind of affinity; the less oxydable metal an increase of the opposite affinity. The pieces of metal, before contact, possessed each a certain portion of elec-

tricity, in common with other bodies, and also a certain intensity of the common attribute affinity: and these powers were balanced as in all other cases. When the contact was established, each metal acquired an increase of its proper affinity: we may therefore easily conceive, that the balance of the antagonist powers being destroyed, and the peculiar affinities predominating, there might be such a disturbance of the repose of the electric fluid as would cause it to appear in a sensible form, upon so perfect a conducting substance, after the separation of the metals. While the contact subsists, the electricity must remain in its natural state, on account of its tendency to equilibrium; but the instant the contact is broken, the transferred affinity is withdrawn, and the difference of electric state remains. For the electricity being the effect of a tendency commenced subsequently to the transfer of affinity, that tendency must continue until the transfer which occasioned it cease; and then, being no longer prevented from exercising its functions by the contact of the metals, it does not continue as a mere tendency to disturb the equilibrium, but that effect is really produced.

As to the peculiar state assumed by each metal, it seems to be not a matter of indifference, but an invariable result: the more oxydable metal is always positive, and the less oxydable negative. We know that since electricity is evolved, both metals cannot remain in the same state, and we

know that the most trifling circumstance is sufficient to determine the electric state of a body. The peculiar nature of each metal might therefore be assigned as the cause that determines which shall be positive, and which negative. But it seems much more probable that it is the peculiar kind of affinity transferred to each metal that decides its state. It is easy to conceive that the increased affinity for oxygen, &c. of one metal may be more compatible with the positive state; and the increased affinity for hydrogen, &c. of the other may be more compatible with the negative. There is nothing improbable in the supposition that, as the affinities transferred are opposite to each other, the electricities evolved should be so equally.

It is probably in the same manner that the two electric states are called forth in the pile. I do not attempt any particular explanation of the manner of the increase of the positive and negative states in the series: the nature of these states is not understood; there is no substantial matter for the foundation, and the superstructure could not be raised without much hypothetical amplification: yet the general cause is probably what has been already stated. The acquisition of one kind of affinity by the zinc throughout the series, beyond what is natural to it, produces a disturbance of its electricity, in a manner specifically the property of the species of affinity so transferred: the kind of electricity thus evolved would be positive. The

copper, receiving an increase of its peculiar affinity, may suffer an analogous change in its electrical equilibrium; and the specific effect of this kind of affinity may be to produce the negative state. There is nothing improbable in supposing each kind of affinity to elicit a state of electricity peculiar to itself; and this determination may be assisted by the peculiar properties of the metals: for bodies are known to have preferences for one kind more than the other: thus sulphur is almost always negative, and renders other bodies positive. On this view of the subject, a reason is assigned why the two states of electricity should co-exist in a connected series of conductors. But on the supposition that electricity is the sole agent concerned, it is quite contrary to all its habitudes that two metals applied face to face should maintain difference of state, especially when of this precise fact there is not one experimental evidence. I have already (p. 162) stated the manner in which I suppose the electricity to be evolved in the pile: the pile is no more than a series of contacts and separations.

That transferred affinity is the cause of the electricity of the pile, has more to recommend it than mere probability: there are facts which it would be difficult to explain otherwise. The principal of these shall be now detailed.

I have stated (12) that the power of transferring affinity was found to be an exhaustible quality; and that a piece of copper, by being exposed, in

contact with zinc, to the action of sulphuric acid, became at length incapable of transferring affinity but in a weak degree. It therefore occurred, if the electrical appearances be occasioned by a transfer of affinity, and if this power of transferring affinity could be impaired, that the power of producing electrical appearances should be proportionately diminished.

A number of pieces of thick sheet zinc were connected each with an equal piece of sheet copper, and were in this state exposed for three weeks to the action of a solution of muriate of soda. They were then taken out and thrown into very dilute sulphuric acid, and suffered to remain a week; and this process was repeated six times with new portions of acid. They were then taken out, and the metals separated: but in order to exhaust them still more, the copper plates were connected with new pieces of zinc, and exposed to new acid until the zinc dissolved entirely. The zinc plates that had been first used were also connected with new copper, and again immersed in very dilute acid. The pieces of zinc and copper that had been thus exhausted were now polished by cautious filing and planishing on a bright anvil, and were then cut into pieces three quarters of an inch square. One hundred and twenty of these were formed into a column, with interposed squares of dry paper, and the column was sustained by glass rods. Another pile was formed of equal number, surface, polish, and from the same

sheets of zinc and copper; but they had not been exposed to the action of any menstruum.

The columns were now alike in every respect, except that one had been to a certain extent exhausted of its power of transferring affinity. I procured a gold leaf electrometer, which, instead of slips of tin foil on the inside, was constructed with moveable slips of brass, that could be adjusted to any distance on each side of the gold leaves, by means of a thumb screw; this instrument was therefore calculated to make comparative trials of the intensity of the two piles. It was in short a real electrometer, and not an electroscope. On the cap of this electrometer was placed the column composed of metal, which had not been exposed to the action of a menstruum: the screw was so adjusted that the gold leaves, in their divergence, could barely strike the brass slips, the top of the pile being connected with the ground. When the distance was found, the column was removed, and the exhausted one was put in its place: the gold leaves were affected, but did not strike the sides, not even when the slips were brought a little nearer. The slips were now brought so near that the leaves could barely strike them: the column was then removed, and replaced by the other: the leaves now struck the sides with violence, and continued to do so when the slips were removed further asunder. To succeed in these experiments, they should not be made in the sun's beams: for al-

though the electrometer in this case possesses great sensibility, yet it never indicates the real intensity of the electricity. I have only formed a conjecture as to the cause: it appears to me that the stratum of warm dry air, in contact with the brass cap, acts as a kind of collector and retainer, like the resinous plate of Volta's condenser. It would be tedious to detail the many reasons on which this conjecture is founded. I satisfied myself that it is impossible, under such circumstances, to ascertain the real intensity of any electrified body. The best time for the experiment is about two o'clock in the morning, when the weather is neither moist nor too dry: and at such a time the results are satisfactory, and invariably as above stated. I found it also more convenient that one only of the gold leaves should strike the brass slip; and this effect is best produced by giving the electrometer a slight degree of inclination. By these precautions, any one may satisfy himself of the fact.

I have witnessed another result of a still more convincing nature, with regard to the diminution of electricity, when the power of transferring affinity is impaired. Five hundred pairs of zinc and copper, each plate containing at each side five square inches, the whole being highly polished, and alternated with dry paper, were erected into a column, by means of glass pillars; and the whole was placed on an insulating stand. The electricity of this column was so strong that it

caused the gold leaves of Bennet's electrometer to strike the sides eight times in as many seconds. Wires terminating in fine points, proceeding from the extremities, when brought together afforded sparks, and, by the usual means, a Leyden phial could be so charged from it, as to give a shock to the tongue. This active column, after standing for eighteen months, had entirely lost every electrical appearance; it did not even produce the slightest effect on the electrometer. The column was then dismounted, the plates were polished anew, and it was again erected with new pieces of paper. It now caused a divergence, but the gold leaves did not go nearer to the sides than a quarter of an inch. I could not, even in the dark, observe the smallest appearance of a spark, nor could a Leyden phial be affected. 120 of these plates were now mounted on the electrometer formerly employed, and compared with the column of small plates which had not been exhausted; the large plates did not produce more than one half the effect of the small.*

Thus it appears, that, in proportion as the

* I know not how to reconcile these facts with the effects of the electric column, which is said to be continual in its action. The plates of the common electric column are not larger than a shilling; the plates of my large column were five square inches: this may perhaps make a difference. It is also possible that the great number used for ringing bells, even although they suffer exhaustion, may possess sufficient activity for the purpose, and therefore the diminution of the effect from time might escape observation.

power of transferring affinity was impaired, the electrical appearances were diminished: and these experiments coincide in a remarkable manner with the supposition which led to them, namely, that the increased intensity of affinity, in the metals of a galvanic series, is the cause of the expulsion of electricity. Since, therefore, these new states of affinity cannot be excited in the metals, without disturbing the electricity that resided in them, may we not suppose (what agrees with every consideration and analogy) that affinity and electricity are antagonist forces?

If this proposition be assented to, it is easy to explain in a different manner, and I think less exceptionably, those experiments which have been considered as establishing the identity of the agent in galvanic and electrical phenomena. I have already attempted to show that these experiments do not prove their object: but I shall now consider them as not connected with any uncertainty, yet otherwise explicable..

If then it be admitted that affinity and electricity are antagonist forces, and if affinity acquired by any body disturb the electricity naturally residing there, so as to cause it to assume an active form, it is no less probable that, conversely, a great intensity of electricity should disturb the natural affinity, so as to cause it to assume a state of activity: for antagonist powers ought mutually to disturb each other, as is the case with affinity and heat. When affinity acts, heat is expelled;

and heat seldom operates, without affecting affinity. In a galvanic series, the most energetic of all affinities is transferred to the polar wires, as is proved by their power of decomposing the most refractory substances: yet a very feeble electricity is produced. Now if such an intensity of affinity can only eliminate so feeble an electricity, it should happen that a very high intensity of electricity should be required to excite a very feeble affinity. This is accordingly the case. Dr. Wollaston, by passing the electricity of a Nairne's machine, with a cylinder 8 inches diameter, through capillary silver wires immersed at a small distance from each other in solution of sulphate of copper, found that the negative wire got a coating of copper. In repeating this experiment I used a new Nairne's machine with a cylinder of $8\frac{1}{2}$ inches diameter, and extremely fine silver wires; but after 3000 turns there was not the slightest change on the wire, further than a little blackness. Although I did not conduct the experiment with sufficient address to obtain Dr. Wollaston's interesting result, yet I considered my failure to afford an argument: it proves that an immense quantity of electricity must pass rapidly over a very small surface, before any chemical change can be effected; that is, the intensity and quantity must be great. This agrees with what is laid down above, that a great intensity is required to excite a very feeble artificial affinity: and it is easy to conceive that, as each kind of

affinity has the power of producing a certain state of electricity, so each state of electricity should produce an invariable kind of affinity. In Dr. Wollaston's experiment, the intense negative electricity continually acting on the silver wire produced that kind of affinity which exists in the negative polar wire of the pile. Were this negative polar wire immersed in solution of copper, the copper would collect round it: in the same manner, when the artificial electricity, passing through the wire, excites that peculiar kind of affinity, the copper is taken from the solution, and attaches itself to the wire. When this coppered wire was placed on the positive side of the machine, the other kind of affinity was excited, and the copper re-dissolved.

But beside the affinity acquired by the passage of condensed electricity through the wire, there is probably, in all cases, another power aiding the decomposition of the compound. We have ample evidence that electricity, under ordinary circumstances, as in sparks or discharges from phials, possesses the power of decomposing certain fluids: thus water, whether in the state of fluid or vapour, may be decomposed. In this manner, the decomposition in Dr. Wollaston's experiments might have been accomplished, but aided by the affinity acquired by the wires. In Davy's experiments also, the electricity might have been principally instrumental in decomposing the compounds, and the excited affinity might have

determined their appearance at the proper pole. In all these cases, I have only attempted to show that the experiments, adduced to prove the electro-chemical hypothesis of galvanism, are explicable according to my views: but I have already offered reasons for supposing that they are inconclusive on the question which they are brought forward to support, and that they cannot be explained by that hypothesis.

So completely opposed to each other are affinity and electricity, that they for the most part mutually disturb each other; and this disturbance is in a certain degree necessary to the operation of one of them. Thus transferred affinity, which is easily obstructed because its presence in any body is a forced state, cannot take place unless electricity can be expelled from the body in which the new state of affinity is excited. Thus non-conductors of electricity, when brought into contact and exposed to a menstruum, do not transfer affinity, nor suffer any change; for the necessary expulsion of electricity cannot take place in them so readily as to be effected by the feeble power of the affinities transferred, and therefore since the antagonist power remains undisturbed, the other cannot exert any influence. This is probably the reason also that galvanic series cannot be formed by any other bodies but conductors; a circumstance which has been looked upon as proving that electricity is the agent in these arrangements. But this conclusion is not necessary, for

beside the explanation which I have offered, it might be supposed that transferred affinity is exerted more rapidly through conductors than any other bodies. This analogy is not between electricity and affinity alone; heat is similarly affected, it passes with ease through most good conductors of electricity, and is obstructed by almost all the bad ones.

SECTION IV.

Of the light and heat manifested by galvanic arrangements.

THE next appearance to be accounted for, on the supposition that affinity is the agent in galvanic phenomena, is the extrication of heat and light.

So completely are affinity and caloric opposed to each other in Nature, that as far as observation leads, it is scarcely possible to effect a change in one, without producing a corresponding change in the other. Instances of this kind are familiar; the heat produced by solid potash and sulphuric acid is great; and even the feeble combination of alcohol with water extricates heat. Light and heat are also continually produced together. Such is the case in the slaking of lime, in the

action of nitrous acid on oils or charcoal. The light and heat attending combinations of oxygen have been considered peculiar to combustion. It is however discovered that other combinations produce the same phenomena, as when copper or iron filings are heated with sulphur in a vacuum. Potassium, with sulphur, or phosphorus, when heated *in vacuo*, produce heat and light: the same appearances attend the combination of platinum and phosphorus. But the most remarkable and applicable instance of all is, the combination of two metals, potassium and tellurium, during which the light and heat are considerable. It is therefore of consequence to examine the circumstances more minutely.

It has been already observed (35), that in all cases of transferred affinity, the effects are more distinct, in proportion as one of the metals in contact is more oxydable. On the other hand, the effects will be still more striking, if the attraction of the other metal to hydrogen be aided by a natural affinity. In the example, therefore, of potassium and tellurium, we have the circumstances for the transfer in the greatest possible perfection: the former has, of all other metals, the greatest affinity for oxygen, and the latter the greatest of all others for hydrogen. Hence, when these are brought into contact, the transfer of the two states of affinity ought to be most considerable. At the moment in which these metals combine, the affinities must quit their state of separa-

tion; they must be supposed again to meet each other, one passing from the potassium, the other from the tellurium, so as to be blended together. From this commixture heat and light result in some manner that it is not possible to understand. In the same manner, it is possible to produce heat and light by the transfer of affinities between other metals. If a very large surface of copper be immersed in an acid, within a short distance of an equal surface of zinc, no other than the ordinary action of the acid on the metals takes place. If the two plates be connected by stretching a wire from one to the other, the affinities are transferred through the wire; it emits light and heat, and fuses. This case is analogous to the former; and, as far as observation leads, no change takes place in the wire, further than suffering the transfer of affinities; so that this transfer may probably be the cause of the ignition. The reason that the simple contact of potassium and tellurium is so much more energetic is, the strong natural affinities of each of the metals to the leading bodies of each class; for such, as we have seen, are always more efficient than acquired energies. The suddenness of the transfer is, no doubt, also a principal cause.

As there appear to be illustrative examples in cases of combination, so also are there others in decomposition. In the latter, the affinities must cross each other as in the former, and we can not much wonder that the same phenomena should

be presented. If tin be immersed in a solution of nitrate of copper, the usual transfer of affinities takes place between the dissolved and the aggregate metal (41), and the former is precipitated. When moist crystals of nitrate of copper are presented to a large surface of tin, I have observed that the same reduction takes place; and heat and light, sometimes considerable, are extricated during the transfer of affinities.

This example is analogous to the preceding; it admits of the same explanation: and by removing an apparent obstacle to the principles laid down in the preceding pages, it gives them some share of probability. To those who consider caloric not as matter but as a motion of matter, there will appear little difficulty in supposing the heat and light to arise from the opposite action of two affinities. If caloric consist in vibrations, these may be easily supposed to arise from such tumultuous action as is observable in large galvanic series. Those who conceive caloric to be matter, and therefore possessed of some of the common attributes of matter, will not be surprised that when affinity is transferred, any subtile matter, so similar in its effects, should also be transferred. But it is really an insurmountable difficulty to understand how a tenuous stream of electricity, so feeble as to be scarcely perceptible to any of the senses, should produce a light and heat which rival the sun itself: and the more especially, as electricity is not known to perform

any thing by its quantity, but by its intensity. We know, as a certainty, that affinity produces the phenomena of heat and light; is there then any thing overstrained in supposing that multiplied affinities should do the same, and often in a higher degree?

That some such tumultuous process as I have supposed takes place in the wire which connects the plates, during the transfer of affinities, appears from a fact witnessed by Mr. Singer. By passing the discharge of a galvanic battery through a wire, that gentleman found that the wire was burst in splinters in such a manner as if by some violent internal action.* Mr. Singer's experiment is quite irreconcileable to the electric hypothesis; for how is it possible that electricity, of the kind supposed to act in galvanic series, could produce such mechanical violence.

The simple case of the two large plates, if extended according to the general principles of galvanic action already laid down, will explain the manner in which a series of plates accumulate these effects. But a material subject of consideration still remains, namely, the influence of number and surface in a galvanic series.

According to the principles already detailed, the superior heating power of large plates over small, and the superior chemical efficacy of many

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* Annals of Philosophy, Octob. 1813.

plates over few, although containing equal surface, may be explained. If a plate of copper and a plate of zinc, each one inch square, be connected by a strap of copper, as in the troughs at present used, the affinities transferred from the plates through the strap are of a certain intensity. If the plates be ten inches square, each square inch transfers its quantum of affinity to the opposite metal, and the sum of the intensity of the affinity transferred is no greater than in the former case. If each of the 10 inch plates be cut into squares of 2 inches, there will be 25 of each metal; and if these be arranged in the form of a battery, the chemical effects will be greatly augmented. Suppose this battery to be represented by fig. 1st, (see the plate) the copper *a* transfers through the wire, and through the zinc plates *e*, *i*, *d*, *c*, *b*, where its action ceases. Thus all the zinc plates have their affinity for the first class of bodies so far increased, and all the copper for the second class. The second copper in the same manner transfers through *b*, *e*, *i*, *d*, *c*; the third through *c*, *b*, *e*, *i*, *d*, and so on, throughout the whole series: thus all the plates have affinity of equal intensity, and each is acted on by all the rest. This explanation agrees with facts long known, namely, that the non-oxydable metal of a series may be of very small surface in proportion to that of the other; a wire has been in some cases found sufficient: but this fact can only hold with regard to small plates in great numbers.

It is also possible to explain the greater heating power of large plates. If a plate of zinc and a plate of copper, each two feet square, be properly immersed in acid, it will be found that a thin wire connecting them will be heated red hot and fused. In this case the affinity of each metal is transferred entirely through the wire, rapidly in proportion as they can be satisfied (22); but in the case above described, the affinities must pass through all the rest of the plates, before they arrive at the wire. Notwithstanding this heating power of one pair of large plates, they possess a very weak chemical energy: but the latter may be increased by cutting the two plates into a greater number. In this case the heating is proportionately diminished, so as to be destroyed if the plates be made numerous. We have already seen the reason why cutting the two plates into a greater number increases their chemical power. The increased intensity, existing in the plates when numerous, must be saturated by the elements of the menstruum with proportionate rapidity, and this continual and violent saturation exhausts the force of the affinities transferred. To prove that transferred affinities, by being satisfied, become less intense, it need only be observed that the same battery which would decompose solid potash, will not do so if the potash be in solution; because the affinities are satisfied by the water as fast as transferred.

Thus in general. When a battery of small plates

is employed, the transfer of affinities from each plate takes place through all the rest; so that the intensity of affinity is increased in each, and therefore their chemical energy is increased. But as these increased affinities must be satisfied with proportionate violence while acting on the plates, their power is exhausted, as shown in the last paragraph, and they do not possess the power of heating bodies interposed in the series. But when the same surface is contained in a few plates, the alternations being less numerous, the intensity of affinity transferred is less exhausted by passing through and being concentrated and satisfied on many surfaces, so that more of it is allowed to act on interposed bodies, and therefore such bodies will be more heated.

SECTION V.

Of the contractions and shock produced in animals by galvanic arrangements.

THE mechanical anatomist discovers the animal fabric to be a machine in which he finds pulleys, fulcra, and levers. The bones move upon their terminal articulations, the muscles attached to them constitute a power which actson a lever of the third order: and the action of the power exists in the shortening and thickening

of the muscular fibre. But here the mechanical observer penetrates no further: the power which shortens the fibre is out of his sphere, and he is obliged to exclaim

"Causa latet, vis est notissima."

If the investigation of this power be within the limits of human observation, it is the province of the chemist. The animal system is an assemblage of the constituents of material things, acting in a state of wonderful harmony: it is a microcosm in which there is a continual process of renovation and decay. These processes are immediately connected with life, and life is the source of motion. It is therefore amongst the laws of chemical change and repose that we are to look for the cause of muscular contraction.

As the body is replete with salts, and as no part is without them, it is plain that these salts perform some essential duty in the animal economy. What this duty is has never been determined, although many conjectures have been formed. To suppose that the salts act as an antiseptic in the destructible fabric of the flesh is a gross conception. The living body requires no such defence, and if it did, the quantity of saline matter would be insufficient, as otherwise a dead body, which also contains this quantity, would be unassailable by putrefaction. It is not unreasonable to suppose that these saline chemical com-

binations are destined to exert chemical effects; and if we suppose that these salts, dissolved in the mass of blood, pass through the circulation of all parts, it is easy to conceive that they act, according to their specific nature, as the appropriate stimulus of particular organs. Such an opinion is countenanced by several facts, and we have instances of artificial saline substances actually producing the effects above supposed. Thus the salts of antimony, sulphate of zinc or copper, and hydro-sulphuret of ammonia, act directly or indirectly on the stomach, causing it to contract and expel its contents: and it matters not whether the primary action be on the stomach or on the muscles connected with respiration, as has been lately asserted by the French physiologists. Almost all the combinations of alkalies with acids stimulate the intestinal canal. Under certain circumstances, nitrate and acetate of potash pass through the circulation until they arrive at the kidneys, to which they are a stimulus. The same salts, with various others, can also be made to act on the exhalant vessels; and many of these salts will produce all the foregoing effects. Thus since we find these chemical compounds capable of stimulating the vital functions, and since we know that anologous compounds are elaborated in the process of nature, may we not suppose that the natural salts are destined to fulfil the ends which we have supposed?

But this is mere probable speculation: there

are facts, however, which show that the muscular fibre is sensible to chemical stimulus of the weakest kind. When two different metals are properly applied to a muscle, it contracts and moves the dependent limb. I have shown that in no case but where one of the metals can suffer chemical action does this contraction take place. Aldini produced contractions by the application of another kind of animal matter; but he found it necessary to use also solution of muriate of soda, a salt which acts powerfully on the muscular fibre; for when used to counteract the septic tendency of flesh, a considerable and permanent contraction of its dimensions follows. Humboldt took the heart of a fish which had ceased to palpitate, and having plunged it into oxymuriatic acid, it began to contract afresh. By laying a drop of sulphuric acid on the sciatic nerve of a recently killed frog, I produced contractions in such quick succession that the limbs appeared to tremble. In these cases no other power seems to have acted than chemical affinity. It cannot be justly supposed that any electricity is generated by a drop of sulphuric acid lying on a nerve: and much less that the heart, entirely immersed in a good conducting fluid, should effect any disturbance in the electric equilibrium. We are therefore compelled to admit that these contractions were produced by chemical action of some kind, and that electricity could have acted no part in them. I have already shown that large and continued

streams of electricity, provided there was no interruption of the circuit, did not in the least affect the limbs of an irritable frog, nor those of other animals. Since then this immense quantity was inactive, how is it possible to suppose that the electricity, if any could be produced, of one drop of acid or of a bit of zinc could cause a powerful contraction?

When there is an interruption of an electrical circuit, in which the limbs of a frog form a part, the passage of the smallest spark will cause contractions. To explain this fact, it is necessary to revert to a principle which I have already endeavoured to illustrate. If electricity and affinity be antagonist forces, and if the increased action of one, in any body, subvert the mutual balance of these powers, it should happen that the repose of the other will be disturbed. By this principle, the contractions above mentioned are easily explained. When the electricity passed through the limb, in a continuous stream, it met with no obstruction; for the animal part is one of the best conductors of electricity, and much better than the fluid which it contains, or than any other fluid: hence the electricity could not assume any efficient intensity. But when an interruption in the condensing circuit was made, the electricity continued to accumulate on the conductors, until it possessed sufficient intensity to pass the interruption: this intensity was therefore capable of effecting some momentary action on the balanced

affinities of some of the chemical combinations, existing in either the animal matter or saline juices of the frog; and we know that the smallest chemical change is capable of causing contractions.

The subject is not confined to this explanation; it may be viewed in another manner. We might consider the electricity passing through the internal structure of the delicate nervous fabric to act as a mechanical stimulus, and the violent passage of so penetrating and active a fluid might be easily conceived to produce the effect. I have often observed, that pulling the nerve, touching it with a bit of glass, with the finger, or with any thing, causes contractions. This explanation is more easily conceived; but I consider the former as more consonant with the mutual agency of electricity and affinity.

The galvanic shock will set the preceding arguments in a still clearer point of view: for it will be allowed that the shock and muscular contractions are modifications of the same effect, and that they are produced by the same cause.

To prove that the shock of the pile is not electrical, arguments have been already adduced. To suppose that a pile of 40 pairs of plates, which, when well excited with tolerably strong nitric acid, will give a shock, yet will not affect the electrometer, is quite incompatible with our knowledge of electricity; and that the fluid evolved cannot act by its quantity is proved by laying a

hand on each conductor of the most powerful Nairne's machine, for although in brisk action, it produces no effect. On the other hand, the plates may be so numerous as to cause strong attractions and repulsions, yet if chemical action be not going forward in the arrangement, there will be no shock. It is therefore plain that chemical action is the cause of the shock, and it has been already shown that chemical action is capable of producing contractions, when there cannot be the slightest suspicion of the presence of electricity: it is therefore probable that the cause of both phenomena is the same.

In order to receive a shock from the pile, the hands must be wet. We have seen that when water is placed between the wires of a galvanic series, it suffers a change of its affinities, and the force which occasions this change is such as to overcome the affinities of bodies which never yielded to other means. The moisture of the hands forms a communication with the fluids of the body; and when the extremities of the pile are touched, the affinity of these fluids must suffer a change like any other compound body similarly exposed. It is therefore no wonder, when the smallest chemical action will cause contractions, that the most violent of all chemical action should cause a shock; especially as the transfer of affinities, acting through the animal circuit, would be capable, were a wire substituted for the body, of heating it to whiteness, of fusing

and dispersing it into globules: A cause which produces such violent effects should be expected, when acting on the delicate fabric of the nerves, to produce a proportionate sensation.

General Recapitulation. Conclusion.

I HAVE now come to the end of my proposed task. A sketch has been presented of the various facts and opinions which have been brought forward from the first developement of this branch of knowledge to the present time. The principal of these opinions were then brought under examination, and from the numerous objections which applied to them, it was inferred that none of them corresponds with phenomena. In the last part, I have offered those views which appeared, in certain points, less exceptionable. The foundation of this view is, that all species of attraction agree in not confining their effects to the body in which they inhere, but that they also act upon other matter. Coinciding with this principle, it has been shown that when two metals in contact are made to undergo chemical action, their affinities are very different from what they are when the metals are separate. While in contact, each metal acquires from the other an increase of affinity of a different kind, and this change is capable of being propagated through a number of metals, properly arranged in contact. This transfer of affinity is what I conceive to produce all galvanic phenomena. When concen-

trated in a pile, it subverts all weaker affinities, and decomposes all compounds. While this power was supposed to depend on electrical attraction, the opinion was surrounded with difficulty : it was acknowledged to be so like the operation of affinity that the identity of the two powers was inferred. According to my view, there are no such difficulties ; nor is there a necessity for admitting this incongruous identity. As the resemblance of the galvanic agent to affinity is so great as to manifest no difference, I infer that they are the same. Since then the conducting wires of a pile can be thrown into artificial states of affinity, it is not surprising that they possess such irresistible decomposing powers : and since there is reason to admit the existence of the two states of affinity, as well as of magnetism and electricity, we are at no loss to assign a reason why the elements of all bodies arrange themselves, with respect to the galvanic series, in a determinate manner. We also find no difficulty in conceiving that when these most energetic affinities are acting, heat and light should result : for these two latter agents, and the former, bear to each other the relation of cause and effect. And since the smallest chemical stimulus produces a contraction of the muscular fibre, it is not surprising that the most active kind of affinity should occasion a shock in an animal. Finally, as there have been reasons adduced to show that so energetic and universal a power as affinity could not safely exist in nature,

without the control of counteracting powers, and as it has been rendered probable that electricity is one of these, it is easy to conceive that antagonist forces should also produce each other when their mutual balance is subverted. Hence when affinity predominates in a great degree, electricity is eliminated in a small degree, and manifests its properties: and conversely when electricity in a great degree predominates, affinity in a small degree is found to exert its energy.

It appears to me that this mode of explaining galvanic phenomena is a simple inference from facts, in which no new agent is assumed, and by which very little innovation is made in the established laws of affinity. I do not pretend that the particular explanations are such as will stand the test of strict and universal application; it is more probable that they will require many alterations and modifications, and little else could be supposed. The completeness which might be expected in the received doctrine ought not to be expected in mine. The former is the work of years, the joint production of men of the first reputation in science, altered, improved, and harmonized, according to the objections and suggestions which its original imperfections called forth. The latter is the work of an unassisted individual, a rude outline, as yet new, founded upon inferences at variance with common opinion, and having to combat with preconceived notions, which really seem to have given a peculiar bias

to the minds of philosophers. In that natural eagerness with which one endeavours to support a favourite opinion, I have been perhaps often led into erroneous conclusions: but I hope less frequently into a mistatement of facts. The only claim on attention that I pretend to is, having brought into view some principles which, flowing from a number of simple facts, may be applied to the explanation of others that are more complicated, and may so far be considered theoretical. If the old well known principle of affinity be sufficient, it were unphilosophical to call in the aid of a galvanic or an electrical agent: or to suppose that electricity and affinity are identical powers, than which nothing can, in my mind, be more repugnant to the properties of each. Because electricity appears in galvanic experiments, and because in a few cases it decomposes bodies, are we to assert that electricity is affinity? As well might we maintain that caloric is affinity; for caloric causes combination and decomposition; it causes light and heat. As well might we maintain that caloric is electricity; both are conducted and obstructed by the same bodies; both have attractions and repulsions; both produce sensations painful to the nerves; both produce light and heat. As well might it be supposed that electricity and magnetism are identical; both have poles which bear to each other a strict analogy; both will repel the similar, and attract the dissimilar pole, in another body: electricity will

convert iron into a magnet; it will destroy a magnet, or change its poles. In all these instances, there are as much grounds for the admission of an identity, as in the case under examination.

In concluding, I have only once more to hope, that whoever attends to the suggestions brought forward in the foregoing Essay, may be uninfluenced by his preconceived opinions, in order that he may more fairly judge whether or not there be any probability in mine.

FINIS.

Index of Names to the History.

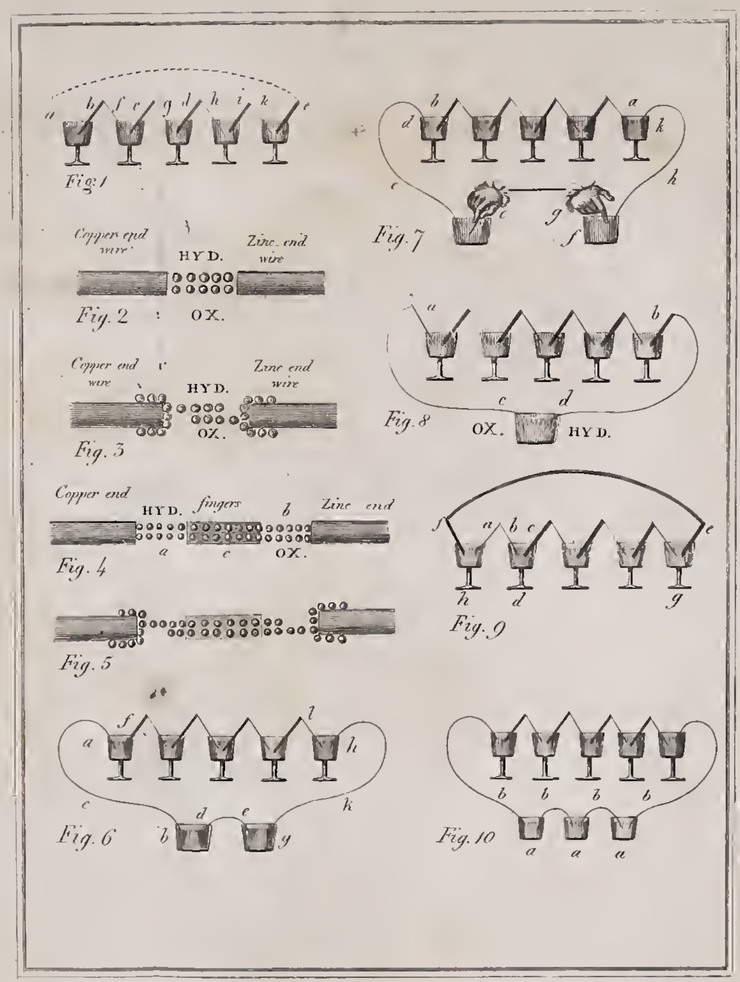

Fig: 1
a b f c g d h i k e
Copper end wire
HYD.
Zinc end wire
Fig. 2
OX.
Copper end wire
HYD.
Zinc end wire
OX.
Fig. 3
Copper end
HYD.
fingers
b
Zinc end
a
c
OX.
Fig. 4
Fig. 5
f
l
a
h
c
d
e
k
b
g
Fig. 6
b a d k h c e g f
Fig. 7
a b
c d
OX.
HYD.
Fig. 8
f a b c e
h d g
Fig. 9
b b b b
a a a
Fig. 10